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OF

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AND

THE COLLATERAL BRANCHES OF SCIENCE.

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> Homo naturæ minister et interpres tantum facit et intelligit, quantum de naturæ ordine, re vel mente, observaverit; nec amplius scit aut potest.
>
> FRANCIS BACON.

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THE

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AN ACCOUNT OF A CASE OF FRACTURE AND DISLOCATION OF THE SPINE.

BY GEORGE HAYWARD, M. D.

[Communicated to the Editors of the N. E. Medical Journal.]

THE following case of fracture and dislocation of the spine, with the account of the operation, which I had an opportunity of seeing while in London, may be interesting to some of your readers. A man aged about 26 years, was admitted in the afternoon of Wednesday, June 15, 1814, into St. Thomas' Hospital, for a fracture of the spine, occasioned by a fall from a house. He was bled previous to his admission, and some imprudent attempts were made to relieve him by pressing the knees against the injured part, which only increased the pain and inflammation. Shortly after he came to the Hospital he was examined by Mr. Cline, Jr. and it was ascertained that some of the spinous processes (probably those of the seventh, eight, and perhaps ninth dorsal vertebræ,) were broken off and were pressing upon the spinal marrow, so as to produce complete loss of motion and sensation in the lower extremities. The patient could not feel when they were pricked or cut, and they were perfectly insensible to heat or cold. This is remarked, because it sometimes happens in cases like the present, that there is a morbid sensibility to the changes of temperature, while there is a complete insensibility to all other stimuli. As it was certain that the man must die if nothing was done to relieve him, Mr. Cline resolved to cut down and remove the pressure from the spinal marrow. On the night of his admission the patient slept tolerably well, and on the day following at twelve o'clock, the operation was performed. An incision between four and five inches in length was made in the direction of the spine, and directly over the injured part. Two spinous processes which were broken off and pressing on the spinal marrow were removed, and a portion of another, probably that of the sixth or seventh dorsal vertebra was cut away so as completely to remove all compression. As the upper vertebra were dislocated and pressing forward, it was thought necessary in order to take off all pressure to remove portions of the transverse processes of each side, which was effected by means of a saw. Several attempts were then made to elevate the vertebra and bring those that were protruded, to their original situation. These however proved fruitless, owing no doubt to the powerful action of the muscles particularly that diaphragm and psoas muscles. As the patient became considerably exhausted, the hope of elevating the depressed portion of the spine was abandoned; the wound being dressed and an opiate given, he was put to bed. After the operation his pulse was 120, but tolerably regular and strong. He complained of considerable pain in the part which prevented him from sleeping as well as on the preceding night. An hour or two after the operation, he thought he perceived a little sensation in the groin of each side and on the superior part of the thighs. This however was probably the effect of his imagination, as he was led to believe that the sensibility of the parts would be restored if the operation was successful. When questioned afterwards as to this point, he said there was no sensation whatever. On the 17th, his pulse varied from 125 to 130, and was much weaker. The contents of the rectum and bladder were passed involuntarily. On the 18th, the pulse was 140, and very weak and fluttering; frequently it was impossible to count the pulsations they were so feeble and indistinct. In the morning subsultus tendinum came on, which increased through the day. The bladder and rectum were evacuated as on the preceding day. On the morning of the 19th, the patient died, and on an examination of the body by Mr. Cline, it was found that the spinal marrow was entirely divided.

From the history of this case and operation, though it terminated fatally, considerable advantage, I think, may be derived, and we ought not to draw any inferences from it unfavorable to similar attempts. The injury was so severe that it was impossible to give relief by any operation; at the same time it proves that the spine will bear very considerable violence without any dangerous consequences. Mr. Cline observed afterwards that in a future operation of the same kind, he should think less caution necessary, and that he should hereafter freely make use of a chissel to remove the different portions of the spine. Though many may think it highly necessary to attempt to remove pressure from the spinal marrow under circumstances similar to those above related, yet some will question probably the propriety of endeavouring to bring the protruded portion of the spine to its former situation. As fractures not only of the spinous processes, but also of the bodies of the vertebra frequently attend dislocations, difficulties will no doubt occur in every attempt of this sort. If the bodies of the vertebra could be replaced, it may be doubted whether they could be kept in their situation, for there is no force to counteract that of the muscles, which are stimulated to contract with unusual violence. Should the fracture of the body of the vertebra be in right angles to the spinous process, it will be completely impossible to retain the protruded portion in its situation; and should it even be in the axis of the spinous process, we cannot hope much, though the case is certainly more favorable. The object of bringing the bodies of the protruded vertebra to their former situation after the pressure is removed, is merely to strengthen the spine, so as to enable it more easily to support the superencumbent weight, and to prevent the deformity which would otherwise take place.

These suggestions are merely thrown out with the hope that this subject will arrest the attention of Surgeons; and should favorable opportunities offer in this country, it it is believed that there will not be wanting those who will be prepared and willing to attempt to give relief in a similar way. The plan of this operation was, I believe, first suggested by Mr. Abernethy, though Mr. Cline deserves much credit, as being the first who has had the boldness to attempt it. Similar cases will no doubt present themselves to him, and from his science, caution and skill, the faculty have a right to anticipate the happiest results.

EXPERIMENTS ON THE INFLUENCE OF THE BRAIN IN THE PRODUCTION OF ANIMAL HEAT, AND IN THE SECRETIONS.

THAT the action of the heart, and the circulation of the blood are maintained independently at least, of any direct influence of the brain, is well established. But the agency of this organ in the production of animal heat, and on the secretions, had not been particularly investigated, till Mr. Brodie of London, made it the subject of experiment.

The result of his experiments seemed to prove, 1st. that an animal body deprived of the influence of its brain, suffered a gradual loss of temperature, notwithstanding, by means of artificial respiration, the action of the heart, the circulation of the blood, and its chemical changes were effected; of course, without the influence of this organ, the functions of the body cannot be maintained for any considerable time. This may be admitted as a fact sufficiently demonstrated. 2dly. That an animal body, thus situated, cools faster in consequence of the artificial respiration, than a dead animal, whose lungs were not inflated.

If this were true, it would necessarily follow, 1st. that animal heat is not the effect of the chemical changes, which take place in the blood; and, 2dly. that no animal heat is produced without the agency of the brain.

Respecting the secretions, these experiments seemed to prove, that so far as the failure in that of the urine is evidence, they are not accomplished without the agency of the brain.

Dr. Hale of this country, repeated Mr. Brodie's experiments, and with respect to the effects of artificial respiration, he obtained very different results. They were published in his inaugural dissertation for the degree of doctor of medicine in 1813.

In my experiment, the effects of artificial respiration were the same as in those of Dr. Hale. And they exhibited other phenomena very different from those of Mr. Brodie's.

I began with young animals with a view to the effects of age on the phenomena.

EXPERIMENT 1st.

CASE 1st.

Cat seven days old, spinal marrow divided by a sharp probe between the occiput and the first vertebra of the neck. Respiration immediately ceased, but followed by gaping. The trunk much agitated for a few minutes. The gaping and sensibility of the trunk lasted 12 minutes. Temperature of the room 62°. A small hole cut through the abdominal muscles, and the bulb of the thermometer introduced into the obdomen. In ten minutes after pithing the mercury stood at 92°. In twenty minutes at 88°. In thirty minutes at 85°. Forty minutes 82½°. Fifty minutes 79°. Sixty minutes at 76½°. Seventy minutes 74°. Eighty minutes 73° In an hour and a half 72°.

CASE 2d.

A cat of the same litter, age and size ; spinal marrow divided in the same manner, as the first ; exhibited similar phenomena. The trachea and carotids laid bare, a hole cut in the trachea and artificial respiration commenced in ten minutes after the division of the spinal marrow. The carotids were full and black, but became red by three or four strokes of the piston of the syringe. The respirations averaged about forty in a minute ; the heart beat with force, and the blood readily changed from red to black, and vice versa, on stoping or renewing the inflation of the lungs. The temperature of the room was 62° ; that of the abdomen of the animal, in ten minutes after the pithing 92°. In twenty minutes 89°. In thirty minutes 87½°. In forty minutes 85½°. In fifty minutes 84.° In sixty minutes 83°. Seventy minutes 82°. Eighty minutes 79½°. In an hour and a half 78°. Means being then taken to prevent hæmorrhage, the head was seperated from the body. Violent agitation for some minutes was the consequence of this operation, and the animal appeared to retain its vital powers in as high a degree as before ; in ten minutes from the best observation, the thermometer had not fallen half a degree ; this circumstance could be attributed to no other cause, than the great muscular motion produced by the operation. It afterwards fell at about the same rate as before the separation of the head.

The syringe used in this and the following experiments was similar to that of Le Gallois. It was prepared for this purpose, and enabled me to measure exactly the quantity of air used at each inflation. It was about seven lines in diameter, and the piston in this case moved from half an inch to an inch; so that the quantity of air used varied from an eighth to a quarter of a cubic inch of air at each inflation of the lungs.

CASE 3d.

Cat eight days old, in better flesh and more recently fed, than the preceeding. Spinal marrow divided in the same manner, but before it was accomplished, the animal struggled much, and afterwards the agitation was more violent and continued longer, than in the last case. Artificial respiration began in nine minutes after the pithing. The themometer stood in the room at 62°; in the abdomen of the amimal in ten minutes 94°. In thirty minutes 88°. In forty minutes 86°. In fifty minutes 85°, In an hour 83½°. In seventy minutes 82°. In eighty minutes 80½°. In an hour and a half 80°. The animal still retained acute sensibility; the heart beat distinctly; carotids full and active, and the blood readily changed its colour by interrupting the respiration. But the animal was devoted to purposes foreign to this subject.

Twelve minutes after the commencement of this experiment the bladder was drawn out, through a hole in the abdominal parietes, and by pressure completely emptied. In an hour and fifty minutes it contained over half a drachm of water.

It may be observed, that this animal, though it maintained a higher temperature, lost in the same time, the same number of degrees of heat as the last. The two degrees by which, at the first observation, it exceeded the last animal are to be attributed to its action immediately before the division of the spinal marrow. But as the higher the temperature, the greater the loss in a given time; other things being equal, the maintainance of this excess must be attributed to its greater vital powers. Some allowance must be made for the cooling at first; in consequence of the intestines being thrust through the hole in the abdomen by the contraction of its muscles.

EXPERIMENT 2d.

Animals decapitated.

CASE 1st.

A cat seven days old, and of the same size and litter as the first animal. Spinal marrow divided at the first vertebra. Carotids, and jugular veins were then tied, and the head seperated from the trunk. The intervertebral arteries were secured, and very little hæmorrhage followed. The head continued to gape a few minutes after its separation. Artificial respiration began in ten minutes after the animal was pithed. The heart beat with force, about 180 in a minute, and the sensibility of the trunk was manifested by the constant motion of the muscles. Temperature of the room 62°. The heat in the abdomen of the animal, ten minutes after it was pithed, 92°. Twenty minutes 89°. Thirty minutes 87°. Forty minutes 85°. Fifty minutes 83½°. Sixty minutes 82½°. Seventy minutes 81°. In an hour and a half 77°. Respiration ceased, and the bulb of the thermometer introduced into the thorax through a small opening, the mercury did not stand so high by half a degree, but in ten minutes at the same height in the thorax and abdomen. The pulsations of the heart had fallen to 130, but its force and the sensibility of the trunk had not materially diminished.

This animal cooled faster than the second of the same size in the first experiment, and the cause was, probably, the loss of blood, though small, which it suffered. The cooling of this, however, offers a striking contrast with that of the first, on which artificial respiration was not performed.

CASE 2d.

A cat of the same age and size as the last in the first experiment. Spinal marrow divided and decapitation performed, as in the first case of this experiment, except, there was considerable hæmorrhage from the intervertebral arteries. Artificial respiration commenced in nine minutes. The heart beat with force, and the blood readily changed its colour on the respiration being interrupted or renewed. The temperature of the room 62°. The heat in the abdomen in ten minutes after it was pithed 94°. Twenty minutes 91°. Thirty minutes 86½°. Forty minutes 84°. Fifty minutes 81½°. In an hour 79°. Here I was unfortunately obliged to interrupt the experiment. The circulation went on at this time perfectly well, and the sensibility of the trunk very little diminished. The rapid manner, in which this animal cooled must be attributed entirely to the loss of blood after the decapitation.

CASE 3d.

A cat nine days old, and a little larger, submitted to the same treatment, but unfortunately lost more blood, than was intended.

This circumstance prevented the process being continued so long as was wished; but it was sufficient to show the effect of the hæmorrhage on the temperature of the animal. Artificial respiration began in eight minutes. The heart beat distinctly, and the circulation and chemical changes of the blood, were effectual. Themometer in the room at 69°; in the abdomen of the animal in ten minutes after the division of the spinal marrow, 95°. Twenty minutes 92°. Thirty minutes 88°. Forty minutes $83\frac{1}{2}$°. Fifty minutes 80°. Circulation still went on, though very feebly, but the sensibility of the trunk had almost disappeared.

If we compare this case with the first, in the first experiment, it will be perceived, that a greater number of degrees of heat was lost in this, in the same time, with an atmosphere seven degress warmer, than in that on which respiration was not performed. Was there then any heat eliminated in this case? If the answer be fairly made from the result of the comparison with the dead animal, it would be none; notwithstanding the chemical changes of the blood, and of course, I presume, some of the vital functions were performed. But the comparison ought to be, not merely with a dead animal; but with a dead animal having its lungs inflated with air above twenty degrees colder than its own temperature. For this purpose the following experiment was made.

EXPERIMENT 3d.

Spinal marrow destroyed.

A cat of the same age and size as the above, had its spinal marrow divided, and in ten minutes after the heat of its abdomen was 94°; that of the air 64°. Spinal marrow immediately destroyed through its whole length. Sensibility of the trunk instantly annihilated. Artificial respiration commenced, but the blood did not circulate; the carotids were empty in five minutes after the destruction of the spinal marrow. Twenty minutes after the animal was pithed, thermometer 90°. Thirty minutes $84\frac{1}{2}$°. Forty minutes $80\frac{1}{2}$°. The inflation was no longer continued.

This animal was most completely dead, and the effect of blowing cold air into its lungs is very striking, if we compare the rapidity of its cooling with that of the dead animal, whose lungs were not inflated. It cooled also faster than the preceeding, making all allowance for the difference of the temperature of the air. Hence it follows that some heat was elaborated even in low state of vitality of that animal.

EXPERIMENT 4th.

The piston of the syringe had not been moved, more than an inch in any of the preceeding cases; but strict attention had not been given to the exact quantity of air used in each inflation. It was desirable to know, whether the quantity materially affected the results.

CASE.

A cat nine days old, and in a good state; spinal marrow divided in the usual place. Artifical respiration began in five minutes. Piston of the syringe for the first forty minutes moved one inch; afterwards only half an inch. Number of respirations forty in a minute. Temperature of the room 69°. Heat in the abdomen in ten minutes 97°. Pulse 180. Heat in thirty minutes 93. Forty minutes 91°. Fifty minutes 89¼°. Sixty minutes 88°. Seventy minutes 86°. Eighty minutes 85½.° In an hour and a half 84½°. Pulse 150. Heat in one hundred minutes 83¾°; in the thorax 83½°.

The functions of the animal seemed to be performed with quite as much vigor, with the less quantity of air, as with the larger. I suspect therefore that a quarter of a cubic inch of air is more, than is consumed by animals of this size. The excess probably would assist in the reduction of the temperature.

This case being of the same age and size as the two last, affords us an opportunity of observing the difference in the cooling of an animal possessing in a good measure the powers of life, and one almost dead from hæmorrhage, and another quite dead, but having its lungs inflated.

The following table exhibits a comparative view of the temperature of all these cases at corresponding periods.

Time after pithing	Dead animal		Animal with artificial respiration.		2d. Animal with artific. respiration.		Animal decapitated art. respiration.		2d. animal decapitated lungs inflated		3d. animal decapitated lungs inflated		Dead animal with lungs inflated		Animal with art. respr. air measured	
	Thermometer in the air.	Therm. in the abdomen	in air	in the abdomen	in air	in the abdomen	in air	in the abdomen	in air	in abdomen	in air	in obdomen	in air	in abdomen	in air	in abdomen
10 min.	62°	92°	62°	92°	62°	94°	62°	92°	62°	94°	69°	95°	64°	94°	69°	97°
20		88°						89°		91°		92°		90°		
30		85°		87½°		88°		87°		86°		88°		84½°		93°
40		82½°		85½°		86°		85°		84°		83½°		80½°		91°
50		79°		84°		85°		83°½		81½°		80°				89¼
60		76½°		83°		83½		82°½		79½°						88°
70		74°		82°		82°		81°								86½
90		72°		78°		80°		77°								84½°

In most of preceeding and in the following experiments, the surgeon's thermometer with a bare bulb, and bent stem was used.

The next question was, whether older animals would give different results.

EXPERIMENT 5th.

CASE 1st.

A cat 14 days old, the largest and most fleshy of three of the same litter, had its spinal marrow divided, by introducing a sharp probe between the occiput and first vertebra, the skin being previously divided, and was suffered to die. The temperature of the room 70°. Heat in the abdomen in five minutes after the operation 100½°. Sensibility lasted eight minutes. Heat in ten minutes 99°. Twenty minutes 96°. Thirty minutes 94°. Forty minutes 91°. Fifty minutes 89°. Sixty minutes 87°. Seventy minntes 85½°. Eighty minutes 84°.

CASE 2d.

A cat of the same age and litter, but thinner in flesh. Pulse 180. Spinal marrow divided in the same manner. Thermometer in the room 70°, in the abdomen of the animal in five minutes 100°. Artificial respiration began in eight minutes; piston of the syringe moved half an inch and forty times in a minute. In ten minutes the thermometer in the abdomen stood at 99°. Twenty minutes 96°. Thirty minutes 94°. Forty minutes 91½°. Fifty minutes 90°. In an hour 88°. Seventy minutes 87°. Eighty minutes 86½°. Pulse 100. Thermometer in the thorax introduced under the top of the sternum at 86¼°. After the first struggling was over, this animal exhibited but little sensibility, and feeble powers during the whole process.

The high temperature exhibited by these two animals at the first introduction of the thermometer was probably the effect of great agitation immediately preceeding, and after the division of the spinal marrow.

CASE 3d.

Animal decapitated.

A cat of the same litter, but one day older. It was in no better flesh than the last, and had eaten very little the last twenty-four hours—pulse 200. Spinal marrow divided, and in five minutes after, the thermometer introduced into the abdomen stood at 97°. Temperature of the room 69°. In eight minutes, artificial respiration commenced; the syringe regulated as in the 2d case. In ten minutes, heat in the abdomen 95°—pulse 160, and feeble.

Twenty minutes, heat 93°. Thirty minutes 91°. The vessels were secured, and the animal decapitated with very little hæmorrhage. Muscles of the trunk violently agitated for three or four minutes. In forty minutes, the thermometer in the abdomen had fallen only to 90½°. Fifty minutes 88½°. Sixty minutes 87°. Seventy minutes 85½°. Eighty minutes 84½°. Ninety minutes 81°—and the same in the thorax. Pulse 120.

The thermometer after decapitation, had fallen only half a degree at the regular interval. This probably was owing to the great muscular motion caused by the operation. It afterwards fell at about the same rate as before.

Water was discharged from the bladder at the commencement of the experiment, and again, when the head was separated. At the end, a considerable quantity was found in it.

EXPERIMENT 6th.

CASE 1st.

A cat eighteen days old, pithed in the usual place and suffered to die. As it presented no remarkable phenomena, a detail is omitted. Its progress in cooling is shewn in the next table at regular intervals.

CASE 2d.

The secretion of urine takes place.

Cat 18 days, and about the same size as the last. Pulse 200. Spinal marrow divided and artificial respiration began in eight minutes after. In ten minutes the thermometer introduced into the abdomen stood at 97°. Temperature of the room 64. Number of inflations in a minute from 40 to 46; the syringe moving half an inch during the first hour, the remainder of the time an inch; pulse 160. In twenty minutes, thermometer in the abdomen 94½. Thirty minutes 93°. Forty minutes 91°, pulse 180. Forty minutes thermometer 89°. Sixty minutes 87½°. Seventy minutes 85½°. Eighty minutes 84½°. Pulse down to 140 in a minute. Thermometer in an hour and a half 84°, and in the thorax the same.

In about 12 minutes after the animal was pithed, the bladder was drawn out of the abdomen, and completely emptied; at the end of the process it contained at least a drachm of water.

Table shewing the comparative cooling of the above animals.

Animals 14 days old.						
	dead animal		animal with artificial respiration		2nd do. and decapitated	
Time after pithing	Therm. in the air	Therm. in the abdomen	in air	in abdomen	in air	in abdomen
5 min.	70°	100½°	70°	100½°	69°	97°
10		99°		99°		95°
30		94°		94		91°
40		91°		91½°		90°
60		87°		88°		87°
80		84°		86½°		84½°

Animals 18 days old.				
	dead animal		animal with artificial respiration	
Time after pithing	in air	in abdomen	in air	in abdomen
5 min.	64°	97°	64°	97°
		92°		93°
		90°		91°
		86°		87½
		82°		84½
90		81°		84

EXPERIMENT 7th.

CASE 1st

Cat 4 weeks old. Spinal marrow divided. It struggled much previous to the operation, and was violently agitated after it. Sensibility lasted five or six minutes. Temperature of the room 70°. Heat in the abdomen of the animal in ten minutes 100°. Twenty minutes 97°. Thirty minutes 94°. Forty minutes 92°. Fifty minutes 90½°. Sixty minutes 89°. Seventy minutes 87°. Eighty minutes 85½°. In an hour and a half 84½°.

CASE 2d, ANIMAL DECAPITATED.

Urine secreted.

Animal of the same litter, age, and nearly of the same size. Pulse beating at least 180 times in a minute. Spinal marrow divided, and artificial respiration commenced in five minutes. Pulse fell to 120. Number of respirations 46 in a minute, piston moved an inch. Temperature of the room 70°. In the abdomen of the animal in ten minutes 100°. The vessels being secured, the head was removed in twenty minutes, but not without considerable hæmorrhage. Pulse fell to 100 in a minute. Heat in the abdomen in thirty minutes 96½°. Forty minutes 94½°. Fifty minutes 93°. Pulse risen to 160. Sixty minutes heat 92°. Eighty minutes 90°. In an hour and a half 89°. In two hours 86°. Pulse 110. Immediately after the decapitation, the bladder was drawn out of the hole in the abdominal parietes and completely emptied by pressure. Examined at the end of the experiment, there was found in it about a drachm of water.

CASE 3d—DIGESTION PERFORMED.

An animal of the same litter was permitted to live two days longer, and during this time was well fed. The principal objects of this case were—first, the effect of good living on the temperature; secondly, whether digestion were accomplished in the degree of life, possessed without the influence of the brain. The animal, therefore, was confined, and kept seven hours without food; but when every thing was prepared for the experiment, permitted to eat a full meal, consisting of meat cut into small pieces, which it devoured voraciously, without much chewing. As soon as it had finished its repast, the spinal marrow was divided, and the respiration commenced in five minutes; they were 40 in a minute, and the piston moved from an inch to an inch and a half. The heart before the division, beat about 200 times in a minute; it fell to 130. Temperature of the room 67°. In the abdomen, ten minutes after the division of the spinal marrow 100°. The bladder was emptied. Heat in the abdomen in twenty minutes 99°. Thirty minutes 97½°. Pulse 170. Forty minutes heat 96½°. In an hour 95°. Eighty minutes 92½°. Ninety minutes 92°. In two hours 90°, and the same in the thorax; the bulb of the thermometer introduced under the top of the sternum. The sensibility of the animal still acute; the heart beat with force and about 130 times in a minute, when the inflation ceased.

The stomach, examined two hours from the commencement of the experiment, was full of food. A considerable proportion of the meat remained in such pieces as it was swallowed, but so soft as to be easily reduced to a pulp. In contact with the parietes of the stomach, especially towards the pyloric orifice, it was already in a state of a greyish pulp. In the duodenum was a thinner and whiter substance.

The bladder contained at least one drachm and a half of water.

The greater quantities of air allowed this animal, may account, in part, for the great vigour of its vital powers.

EXPERIMENT 8th—ON DOGS.

CASE 1st.

A dog about 4 weeks old, and large for its age, in good health and flesh, had its spinal marrow divided, as in the preceeding experiment and permitted to die. Gaping and sensibility continued five minutes. Thermometer in the room stood at 68°. In the abdomen of the animal in ten minutes at 100°. Twenty minutes 67½°. Thirty minutes 95½°. Forty minutes 93°. Fifty minutes 91°. Sixty minutes 90°. Seventy minutes 89°. Eighty minutes 88°. Ninety minutes 86½°. In two hours 84°.

CASE 2nd.

A dog of the same litter, age and size; spinal marrow divided, and artificial respiration commenced in three minutes; they were 40 in a minute; piston moved an inch and a half; pulse before the experiment 180 or 190 in a minute, fell to 120. Temperature of the room 68°. In the abdomen of the animal 101½°. The animal had been held for some time against its will, and to its struggling may be attributed this high temperature at first. The carotids were tied, and the eighth pair of nerves divided. In twenty minutes the mercury had fallen to 99°. In thirty to 98½°. Pulse had risen to 165. Heat in forty minutes 97°. In an hour 95°. Eighty minutes 94°. Ninety minutes 93°. In two hours 91°. Pulse 140 in a minute.

More particular attention was given to the state of the pulse in the two last, than any preceeding experiment. Previous to the division of the spinal marrow, accuracy, as to the number of pulsations is not easily obtained, in consequence of the motions of the animal, and the fear by which it is agitated. It evidently, however, sinks very much immediately after the pithing, and rises again after the artificial respiration has been continued for a few minutes.

This Table shews the degrees of cooling of the subjects of the two last experiments.

Experiment 7th.

Times after pithing	dead animal.		animal with artif. resp. decapitated.		animal with artif. resp. pithed only	
	therm. in the air	in the abdomen	air.	abdomen	air	abdomen
10 min.	70	100	70°	100°	67°	100°
30		94		96½°		97½°
40		92		94½°		96½°
60		89		92°		95°
80		85½		90½°		92°
90		84½		89°		92°

Experiment 8th.

Times, &c.	dead animal.		animal with artif. resp.	
	therm. in the air	in the abdomen	air	abdomen
10 min.	680	100°	68°	101½
		95½°		98½°
		93°		97°
		90°		95°
		88°		94°
		86½°		93°
120		84°		91°

The experiments are sufficiently numerous to authorize the conclusions :—first, that an animal deprived of the influence of its brain, but having some of its vital phenomena maintained by means of artificial respiration, does not lose its heat so fast, as a dead animal, whose lungs are not inflated. Secondly, that some heat is generated independently of the agency of the brain.* Thirdly, that the secretions, certainly that of urine, are accomplished without the influence of that organ. These phenomena accord with the doctrine, the result of experiments, lately published by Mr. Le Gallois, that the life of the trunk resides in the spinal marrow.

The third conclusion ought not, perhaps, to be stated without limitation. So far as the division of the spinal marrow removes the influence of the brain, the conclusion is certain; but what the influence of that organ may be on the secretions by the eighth pair of nerves, and possibly in some other way, my experiments have not been sufficiently numerous to determine. In every instance of the division only of the spinal marrow, urine was found at the end of the experiment in the bladder, which had been completely emptied at the commencement. This has not been so uniformly the case, when decapitation was performed. In some cases so much water presented, that it could not be accounted for, except it were secreted during the time of the experiment. In others so little, that it might have accumulated from other sources. Some animals, especially young cats, secrete only a small quantity of water in twenty-four hours; the proportion for one or two hours is not ascertained.

To make good the first conclusion, the quantity of life remaining to the animal must be taken into the account. For the ability of the animal to maintain its heat in this artificial state, appears to be in proportion to its vitality, and to the vigour with which the functions are performed. It may therefore be in such circumstances as to cool faster in consequence of the artificial respiration, though it still retain sufficient life to effect the circulation of the blood, &c. than a dead animal, whose lungs are not inflated: and these results would appear similar to those which occurred in the experiments of Mr. Brodie.

This gentleman decapitated the animals on which he tried the effects of artificial respiration. This operation puts the subject in a state very unfavourable for the developement of any function, which may be independent of the brain; because it suffers from other causes, than the loss of the influence of that organ. Of these, hæmorrhage is the most destructive. The intervertebral arteries are so inaccessible, that it is very difficult to secure them. If it be attempted to tie them before they enter their canals, dissection must be made so deep in the thorax as materially to injure the animal. I have made many attempts to secure them between the first and second vertebra, first cutting through the skin and muscles; and also between the occiput and the atlas; but not always with success. If, however, these and the carotids, and the large veins are well secured, the loss of blood from smaller vessels within the vertebral canal, &c. will often be so great as to diminish very much the remaining powers of the trunk.

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* Because it is proved, that the generation of animal heat is not the work of the brain; it by no means follows, that it is the result of the chemical changes of the blood. In fact, the experiments almost prove the contrary. The chemical changes in the blood appear to be perfectly accomplished, but the temperature is not maintained. This however is a subject which demands further investigation.

The division of the eighth pair of nerves also, will have considerable influence on the condition of the animal, as the function of the lungs will, in consequence, sooner or latter be interrupted. In some cases this has taken place in a short time, and the bromchial tubes, and the air cells have been so filled with a bloody froth as to render the continuance of the inflation almost impossible. In other cases, they have been for an hour, or an hour and a half so little injured, that the production of heat at least has not, apparently, been interrupted by this cause.

The quantity of air used in the inflation of the lungs probably effects the results of the experiment. If it exceed very much the amount necessary to the change of the blood; the excess will have a tendency to cool the animal. If on the other hand too little air be employed, it will cool in consequence of its vital functions not being properly supported. I am convinced, that in the 5th, 6th, and 2d case of the 7th experiment, the quantity of air was insufficient for its appropriate object. The quantity necessary to give the carotids a faint red only, and that which will maintain a high state of vital action are materially different.

The age of the animal does not appear to affect the general results.

Additional experiment on the effect of artificial respiration.

Life to a certain extent can be supported by means of artificial respiration for some hours in a body deprived of the influence of its brain; several of the organic functions being accomplished in this state, as the secretion of urine, digestion, the production of animal heat, and many of the more hidden processes, as is evinced by the change, which the blood undergoes in every circulation. Why these phenomena cannot be continued indefinitely by the same means; or in other words, what causes the death of the trunk deprived of its brain, or what is the kind of influence which the brain exercises to maintain œconomy, the animal remains yet to be discovered. Mr. Le Gallois supposes the artificial respiration to be an influential cause of death. He says in a note in his book on the principle of life, page 220, of the translation, "that a young rabbit in other respects sound and healthy, may be made to perish with cold by pulmonary inflation, if no other passage for air into the lungs, except through the syringe be left open." I took a cat 12 hours old, and subjected it to pulmonary inflation by the syringe. The heart beat 200 in a minute. Every thing being prepared, the bulb of the thermometer was introduced into its fauces, and the mercury rose from 75°, the temperature of the room to 94½°. The inflation then commenced, and continued without interruption thirty minutes; thermometer again put into the fauces stood at 94½°. The piston of the syringe was moved regularly and exactly half an inch, and made from 40 to 46 respirations in a minute. The inflation was continued in this way, till an hour and ten minutes had expired. Heat in the fauces at this time 43½°. I despaired of freezing the animal to death at that rate, and moved the piston an inch. In fifteen minutes after this change the heat in the fauces was only 92½°. In this way its destruction by cold might undoubtedly be accomplished in time; but it would be the effect of an improper use of artificial respiration, and the establishment of the fact on such terms, of little consequence. I therefore discontinued the process, and permitted the animal to breathe. The action of the heart was not perceptibly diminished, nor were there any signs of material injury. The animal having breathed five minutes after the inflation ceased, I divided the spinal marrow, and immediately introduced the thermometer into the abdomen. It rose to 93½°. Pulmonary inflation recommenced. In fifteen minutes from the pithing, the mercury fell to 91°, and continued to fall at about the same rate as in other similar cases.

This experiment seems to prove, that pulmonary inflation is not necessarily a very powerful cause of death; and except the inaccuracies, to which it is liable, that is a good substitute for natural breathing. It also tends to prove, that the loss of the brain causes death to the trunk in some other way besides that of depriving it of the voluntary power of breathing.

Some apology may be necessary for the destruction of so many animals.—If the importance of the research is not sufficient, I have to observe, that for the most part, they were rescued from a watery grave, and devoted to a more useful, and of course a more honorable death.

WM. GAMAGE, JUN.

Since arranging the above paper, I have repeated many of the experiments; one of which I wish to add, as it exhibits a striking instance of the production of animal heat independently of the brain.

The subject—A cat at least eight weeks old; spinal marrow divided, and pulmonary inflation immediately commenced. My intention was to destroy all possible connexion between the brain and the trunk, without hæmorrhage. The carotids were tied; also the intervertebral arteries between the occiput and the atlas: the eighth pair of nerves were divided. A portion of the occipital bone was removed; the medulla oblongata lifted up, and a probe moved along the base of the brain; this produced convulsive motions of the trunk and the lower extremities. The sympathetic, and the spinal accessory nerves were divided. The probe then moved in the same way caused no motion. At the commencement of the experiment the thermometer stood in the room at 76°; introduced into the abdomen of the animal, it rose in ten minutes to 102°. Thirty minutes 100½°. Forty-five minutes 99½°. In the fauces 95°. In the substance of the brain 95°. In an hour and a half in the abdomen and thorax 97½°. In the fauces and any part of the brain at 90°. The head was connected with the trunk by the chain of bones, a few muscles and some skin. The pulse in this case was, previous to the pithing, about 180 in a minute; in fifteen minutes after 180; in an hour 170.

WM. GAMAGE, JUN.

TO THE EDITORS OF THE NEW ENGLAND JOURNAL OF MEDICINE.

GENTLEMEN,

The following short description of an hydrocephalic patient, in a practical point of view, is of little importance. I have made a record of it merely as a subject of curiosity. If it be considered sufficiently interesting for publication, it is at your service.

Yours respectfully,

A PHYSICIAN.

Boston, August 20, 1814.

ABOUT a year ago, I accidentally saw the child of Mr. PETER TWIST, and was struck with the enormous size of its head. Since that time, I have made myself acquainted with the following particulars. The child was six years of age in April last. She was apparently healthy and thriving until she arrived at the age of three months, when the parents observed that the head began to enlarge, and at the same time symptoms of fever to appear. The patient became restless and irritable, particularly at night, and soon discovered signs of debility and emaciation. The violence however of these symptoms gradually lessened, notwithstanding the head continued regularly to increase in size up to the present time, that is, for about six years; and this day, Aug. 20, I have, by actual measurement, ascertained it to be of the following dimensions—

A line carried horizontally around the head in its widest part, and the two ends of which met at the centre of the forehead, measured *two feet and a half.* A cord carried from the root of the nose over the vertex to the most prominent part of the occiput, measured *two feet;* and the vertical circumference, if it may be so called, or a cord passing under the chin and meeting at the crown of the head, was equal to *two feet and a half.*

As the head continued to increase in size, the bones, which had never been united, were gradually separated to a greater distance from each other; the fontanelle, caused by this separation is of course large, and of a very oblong, irregular shape: the frontal and occipital bones, at one part, approaching within an inch of each other, while the distance between the parietal bones is at least *seven* inches. No pulsation can be perceived at this fontanelle. The scalp is covered with a luxuriant growth of light coloured hair. At the first glance of the eye, the form of the head appears not unlike that of the caps which are brought to us from the islands of the Pacific ocean, the forehead being square and jutting over the eyes, and the occiput being considerably elongated.

The height of the child is 36 inches, of which more than one third is occupied by the head. Her naked body measures 21 1-2 inches in circumference.

This poor child lies continually in a cradle, incapable of supporting herself on her feet, or even of turning her head. Notwithstanding all this, she appears to possess all her senses, with the exception perhaps of vision, (as she slightly squints and the pupils are dilated) in the same degree as other children of her age. She talks well, is fond of play, and when any thing amuses her, laughs heartily. Her appetite is good, and with her parents she lives on food, which, in consequence of their extreme poverty, is necessarily coarse. The bowels are regular, but her thirst is great, and the mother says her urine is very copious and fœtid. The pulse when I examined it was 100, and like the respiration, regular. This patient sleeps but little, sometimes not more than two or three hours in the twenty-four, but when she does sleep, she is perfectly tranquil. Occasional paroxysms of pain, extreme restlessness and of heat occur, and these most commonly come on in the night. At such times she suffers exceedingly, and screams out with great vehemence. There is also sometimes a spasmodic state of the extremities, in which the mother says the limbs become rigid and bedewed with a cold sweat. I could not learn that these were confined to one side alone. When she is not thus affected she appears to move all her limbs with great facility.

PART OF A DISSERTATION ON LIFE.

Containing an examination of some of Le Gallois' experiments.

BY LA FAYETTE PERKINS, M. D.

THE following are the experiments which were made to determine the effects of various wounds of the medulla spinalis, on the

CIRCULATION OF THE BLOOD.

1st. Section of the spinal marrow near the occiput.
2d. Decapitation.
3d. Destruction of all the medulla spinalis.
4th. Destruction of the medulla cervicalis.
5th. Destruction of dorsal portion of the medullæ.
6th. Destruction of the lumbar portion of ditto.

His object* in the three first cases, was, to compare the state of the circulation after the section of the spinal marrow at the occiput, and after decapitation, with the state of it after the destruction of the spinal marrow. In the three latter cases, the destruction of the same portion of the medulla, not producing the same effects on life at the different ages, the object of them was to compare their effects, in relation to the circulation every five days.

He has related to us one of the experiments for each of the six cases which were performed on the first, tenth, and twentieth days. Those of the first and tenth days I have repeated with great care from two to four times. In which I was obliged to sacrifice between 30 and 40 animals. The following are the results.

EXPERIMENTS ON RABBITS ONE DAY OLD.

CASE 1st.

Section of the Medulla Spinalis near the Occiput.

Circulation continues.

Having cut the medulla spinalis with a needle between the occiput and first vertebra. All the inspiratory motions were instantly annihilated, and replaced by gapings, the animal was agitated about a minute, after which, sensibility continued over the whole body until extinguished at 18′. The gapings continuing and the carotids being black and round, but smaller than at the commencement of the experiment, I began the inflation of the lungs; immediately the carotids grew larger and very red; a little after, the gapings became fuller and accelerated; sensibility was restored at 21′. On interruption of the inflation the carotids became of a dark colour and red on its renewal; at 25′ amputated a foot, the hæmorrhage was of a vermilion colour; at 30′ tied both the carotids with the external and internal jugular veins.

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* Le Gallois.

CASE 2d.

Decapitation.—Circulation continues.

At 32′ the same rabbit was decapitated by cutting between the occiput and first cervical vertebra, pulmonary inflation was resumed at 33′, sensibility still remaining in the trunk and extremities. At 40′ amputated a foot; the hæmorrhage was red or dark as the inflation was either continued or suspended.

CASE 3d.

Destruction of the whole medulla spinalis.

Circulation suddenly stopped.

In the same rabbit at 50′ the same state of sensibility and hæmorrhage continuing, and the pulsation of the heart being distinct through the parieties of the thorax, I destroyed the whole medulla spinalis. Instantly the whole body became flaccid and dead. Throbbings of the heart could no longer be felt. Inflation was resumed at 51′ without effect.

A thigh cut off at 55′ did not bleed, the other cut off at 60′ yielded a very little venous blood which being wiped off with a sponge, bled no more. The inflation was discontinued at 70′, and the thorax opened at 90′. The pulmonary veins were found partly black, and partly red.

To prove that the sudden cessation of the functions of the heart, was not owing in any degree to the loss of vitality during the two first experiments, I took a fresh rabbit and without decapitating, destroyed the whole medulla spinalis. by introducing the probe between the occiput and first cervical vertebra passing it on to the tail. Instantly the whole trunk became flaccid and dead. Gapings were the only signs of life which could be observed in the head; the throbbings of the heart were no longer distinct. At 4′ the carotids being flat and nearly empty, containing only a little black blood, commenced inflation. Both thighs were cut off, one at 6′ the other at 9′ did not bleed; the inflation was continued till the 18′, without success;—colour of the pulmonary veins were found the same as in the first rabbit.

SAME CASES ON RABBITS TEN DAYS OLD.

CASE 1st.

Section of the Medulla Spinalis at the occiput.
Circulation continues.

The medulla spinalis being cut between the occiput and first vertebra, gapings ceased at 3′, and sensation at 5′. At 7′ the carotids being small and black, inflated the lungs, at the 4th stroke of the piston, the carotids became of a bright red, much larger and pulsated strongly. Sensibility reappeared at 8′; at 11′ amputated a foot; there was red or black hæmorrhage as the inflation was or was not continued. At 14′ the same phenomena continuing ligatured the carotids and jugular veins.

CASE 2d.

Decapitation.—Circulation continues.

At 15′ decapitated the same animal, between the occiput and first cervical vertebra; renewed the inflation at 16′. At 18′ sensibility appeared more lively than before decapitation, and the animal was more strongly agitated. At 20′ a foot was amputated; hæmorrhage red. At 21′ the inflation was interrupted, the hæmorrhage became black. At 28′ sensibility appearing extinguished and the hæmorrhage stopped, but the throbbings of the heart distinct, renewed the inflation. At 29½′, the hemorrhage reappeared in more considerable quantity and remained red during the inflation.

CASE 3d.

Destruction of the whole Medulla Spinalis—Circulation ceases.

In the same rabbit at 33′ sensibility being very apparent, the hæmorrhage red and the throbbings of the heart distinctly felt through the parieties of the thorax. I destroyed the medulla spinalis. At 34½′ the throbbings of the heart were not perceptible, nor could they be made to appear so afterwards. Inflation was resumed at 35½. Both thighs cut off one at 36½ the other 40 did not bleed. The inflation was continued with care till 42′ without effect. At 60′ the breast being opened the pulmonary veins were found red.

Another rabbit having been taken and the whole medulla spinalis destroyed, without decapitation, furnished the same results.

CASE 4th.

On a rabbit one day old.

Medulla Cervicalis destroyed.—Circulation stopped.

Immediately after the medulla cervicalis was destroyed, gapings commenced, the neck became flaccid and dead, accompanied by an entire loss of sensibility in the anterior paws; sensibility continued in the rest of the body. The throbbings of the heart were scarcely distinguishable, inflation was begun at 4′ the throbbings of the heart became more distinct. The carotids which were nearly empty, and black, became filled with red blood. Soon after the throbbings ceased to be distinct and the caritods were gradually emptied. At 6′ they contained only a small stream of red blood, which preserved its colour during the interruption of the inflation. Amputated a thigh at 6′ the hæmorrhage was small and black. It continued a few minutes without change of colour. Sensation and gapings ceased at 11′. The other thigh being cut off at 14′ did not bleed. The inflation was discontinued at 16′. On opening the thorax the pulmonary veins were found of a brown colour.

Same case on a Rabbit 10 days old.

The medulla cervicalis destroyed. Inflation commenced at 2½′ The carotids being nearly empty and flat, the throbbings of the heart no longer distinct, gapings and sensibility remaining; a very little red blood returned slowly into the carotids, but far from enough to make them plump. Sensibility and the gapings were extinguished in 3½′. The motions of the heart did not become distinct. Both thighs amputated, one at 4′ the other at 10′ gave no blood. Inflation was given up at 15′.

CASE 5th.

On a Rabbit one day old. Destruction of the Dorsal Portion of the Medulla Spinalis.—Circulation continues.

Immediate destruction of the medulla dorsalis by introducing the probe between the last dorsal and first lumbar vertebra passing it upwards. The head, neck, and hind parts continued alive, the middle part become flacid and dead. The inspiratory motions subsisted in a weak manner, and performed only by the diaphragm. The throbbings of the heart were weakened; there were no gapings. A foot cut off 5¼′ did not bleed. At 6′ amputation of a leg, Red hæmorrhage at 10′. Breathing was accelerated. At 15′ the animal continued to live and breathe, and the hæmorrhages were red.

The same case performed on a Rabbit 10 *days old.—Circulation stopped.*

The medulla dorsalis destroyed. Respiration continued for a short time; afterwards disturbed and carried on only by the diaphragm; a minute and a half after, a leg amputated, which produced red hæmorrhage. At 2′ respiration was succeeded by slow gapings, accompanied by deep contractions of the diaphragm. Inflated the lungs at 4′; carotids still beating, but containing only a small stream of red blood. The inflation had no effect, the carotids continued to empty themselves; sensibility and the contractions of the diaphragm ceased at 5′. The inflation was continued till the 13′ when it was given up. The thorax was opened ten minutes after, the pulmonary veins were found red and the foramen ovale closed.

CASE 4th.

Lumbar Portion of the Medulla Spinalis destroyed in a Rabbit one day old.—Circulation continues.

The whole lumbar portion of the medulla was destroyed by introducing the probe between the first lumbar and last dorsal vertebra passing it to the tail; all the hind parts were instantly struck with death, the rest of the body remained alive, respiration was a little deranged at first, after which it was pretty well restored and performed without gapings. At 8′ one of the feet was amputated which gave red blood; at 15′ respiration continued with sufficient facility, and the throbbings of the heart were distinct. Mr. Le Gallois says, that after this experiment "the animal carries its head well, and supports itself upon its fore legs." This assertion I am rather disposed to think is forced in to make his experiments look well, as well as his very great precision in respect to the duration of the gapings; for in all the rabbits, cats, and dogs of one day old, which I have seen or experimented on, there has not been one who was able to support itself upon its fore legs, even before the experiment, much less after so cruel a one, and the gapings were scarcely ever so exact in their duration, excepting in one case; no one ever opened its mouth to me more than once after its head had been cut off; but there is no doubt but the other signs may be depended on.

The same case of Rabbit 10 *days old.—Circulation continues.*

The lumbar portion of the medulla spinalis destroyed. Respiration was disordered for a short time; at 11′ the respiration was performed freely. At 12′ a leg amputated gave red blood, at 15′ the animal remained in the same state, at which time the experiment being accomplished, and not wishing to prolong its distress; I suddenly ran the probe from the incision up to the occiput, which completed the destruction of the medulla spinalis, and thus dispatched the animal.

Thus far have I repeated the experiments of Mr. Le Gallois. To accomplish which I have not spared either expence or labour.

The results of them are so near the experiments of Mr. Le Gallois, as to leave no doubt in my mind of his correctness.

In all the animals which I lost in performing the experiments, I was able to discover the cause of the deviation: sometimes it was by wind getting into the cavities and compressing the lungs and vessels, or imperfect destruction of the medulla; also I found that the measurements given by Mr. Le Gallois for the different portions of the medulla were not adapted to the animals I had. The consequence of which was, that I lost five or six rabbits. After finding out the cause of failure, I took my own measurement and did well.

I cannot close this dissertation without confessing my obligation to Mr. Charles G. Adams, my friend and fellow pupil, to whose kind and indefatigable exertions in watching the phenomena and noting down their occurrence in every experiment, I am very much indebted for the ease and correctness with which I was enabled to perform the experiments.

HISTORY OF A CASE OF PERIODICAL SINGULTUS, SUCCEEDED BY SEVERE DYSURIA, TERMINATING IN ISCHURIA AND ULCERATION OF THE KIDNEY.

[Communicated for the New England Journal of Medicine, &c.]

BY DR. JOSEPH TORREY.

THE subject of the following case, is a female, twenty-four years of age, of a florid countenance and exquisite sensibibity. She had enjoyed good health till June, 1809, when by taking cold, menstruation was suppressed, and has rarely since recurred till the present time. She has repeatedly experienced severe indisposition from no other apparrent cause than the loss of a relative; and her late confinement she dates from an event of this nature:

I was called to visit her Jan. 21st, 1812, and received the following intelligence respecting her.

Having retired to rest on the evening of Feb. 1811, she was roused from sleep by the news of the sudden death of an uncle near her dwelling, when she arose and repaired to the scene of distress. To this succeeded a fever of some continuance, from which she did not recover to perfect health; but was troubled with dyspepsia, and amenorrhæa.

In March she was attacked with singultus, which continued to recur daily. It continued with less severity and regularity, till November, when it assumed a more fixed, severe, and regular character; commencing precisely at 11 A. M. and subsiding at 5 P. M.

Through the winter she was able to go about house; but her health was feeble, and she suffered much from indigestion.

She had been treated by various physicians with musk and sundry fœtids, and with chalybeates and other tonics, without any apparent advantage.

My first attempt was to break up the morbid habit (which I then supposed to exist) by producing a strong impression on the stomach and neighboring organs. For this purpose I prescribed ℨiij Tinct. Loheliæ Inflat. at half past 10 to be repeated every 10 minutes till it produced vomiting. The only effect which followed was a distressing sensation which continued for a day or two. I then ordered 2½ grs. Opium to be given at half past 10; which had no influence on the succeeding paroxysm.

She then took 4 grs. Oxyd. Zinc every 4 hours till she had taken 16 doses. This only produced nausea and a large fluxion from the salivary glands.

Considering the intermittent character of the complaint, I had recourse to the Cinchona; of which she took ℥i Decoct. with ʒij Tinct.[c] every 3 hours. After using this a short time there was added to each dose as much Aqua Ammon. Pur. as she could comfortably swallow. After a few days, Fowleri Min. Solut. was substituted for the Aqua. Ammon. until she complained that it occasioned uneasy sensations in her bowels. She then took the Ammoniaret. Cupr. for a week; in which time I made one application of electricity, with a design to continue it; but having applied a blister to the region of the stomach, its irritation was so distressing as to excite a febrile commotion, which required exclusive attention for some days; since which she has taken successively the Nitras Argent. and Oxyd. of Bismuth.

April 1. The pulse, in general tolerably full, being now rather tense, I took from her arm ℥xii blood. She bore the operation well; it was finished at 11, and in 4 minutes the hiccup came on.

Soon after bleeding, she complained of pain in her lower limbs, which was succeeded by a slight Catamenial discharge, which had been wanting many months.

May 1. The hiccuping paroxysms though gradually reduced to 3½ hours, still daily recurring, I was determined to make a more thorough trial of opium.

I left her a quantity of pills containing each 1 gr. I directed 4 to be taken at 9 A. M. but from timidity she took but 3 the first time, which neither induced sleep nor prevented the paroxysm.

May 3, 9 A. M. Took the prescribed dose: at 10 fell asleep; continued in that state till 4 P. M. singultus totally suspended; feels, after awaking, extremely irritable, and occasional faintness till evening.

May 4. Took 4 pills with similar effect.

May 5. Took no pills and had no Paroxysm.

May 6. Took no Opium, Paroxysm came on at the usual time.

but lasted only a few minutes. After this the Opium being laid aside, the hiccup recurred daily, inreasing in duration till the paroxysms lasted an hour each day, for several weeks; when she was induced to resume the use of the Opium, and with the same results as before.

The extreme nausea and vomiting which this medicine produced discouraged her and her friends still more, from its farther use.

June 6. I was called to visit her on account of a violent spasmodic affection of her stomach.

I found on inquiry, she had taken some asparagus at dinner. She immediately took Sulphat. Zinc, and threw it up undigested. The spasms and distress soon subsided.

June 8. She began to raise mucous deeply tinged with blood. It appeared to come into her mouth without cough or effort of the stomach. For its relief she took Tinct. Digit. Purp. and Acet. Plumb.; but venesection was found more useful than any other remedy.

After this symptom had in a measure subsided, she was persuaded to try the exercise of riding, which somewhat improved her appetite and digestion; but the hiccup continuing an hour or more each day, I advised her to a persevering trial of the Opium, in such a way as totally to prevent them for a week or more.

Dreading the intolerable sickness which it invariably occasioned, and desiring to elude the sympathies and remonstrances of her connections, who were opposed to its use, she conceived a wish to leave her own home, and to remain at mine, while undergoing the process; that she might be under my more immediate observation and attention.

August 25. She arrived in the midst of a paroxysm, which lasted an hour, as usual.

Aug. 26, 8 A. M. Took 4 grs. Opium, at 9 fell asleep; at 3, P. M. awoke, and after a short space of great uneasiness, a most distressing sickness and retching commenced, for which she took strong coffee, ordinary doses of Opium with Peppermint, Ol. Cinnam. Ol. Absinth. Aether. Oxyd. Bism. &c. but all were equally unavailing; it continued till evening and then subsided, sua sponte.

Aug. 27. Took no Opium, and had no hiccup.

Aug. 28. Repeated the Opium in the same manner and with the same result as on the 26th. Aug. 30. As the 28th.

In the afternoon of the 30th she observed "I have no fear that the hiccups will trouble me any more." On being asked why? She replied, "I feel differently at my stomach, where a peculiar sensation has hitherto impressed my mind that they were not finally removed; but I now feel so free from it, that I am confident they will not return. It it worthy of remark, that herein she has not been disappointed.

She soon after complained of pain in the region of her left kidney, which she said she had felt more or less, since the 28th.

In the evening, it proceeded in the course of the left ureter till it fixed in the pelvis of the abdomen. A suppression of urine took place, which became so distressing by 3 in the morning of Sept. 1, as to require relief by the catheter.

She was now informed that the obstruction was probably occasioned by a small calculus, which had passed from the left kidney through the ureter into the bladder: that it was very important she should unite her endeavours with such medical aid as would be used, to promote its expulsion: and that of course she must use every practicable endeavour to void the contents of the bladder without the catheter, as the use of that instrument would necessarily preclude the discharge of the supposed calculus.

This idea she fully adopted; and to facilitate the discharge, she used the various appropriate remedies; but the use of the catheter was notwithstanding indispensable twice or thrice* in twenty-four hours, till Sept. 7th, when, after a persevering, agonizing effort, a sound was heard as of a small hard substance falling into the vessel over which she was sitting. "Something" said she, "has passed from me." And directly there was a free discharge of urine. On examining the contents of the vessel we found, to our astonishment, two small pieces of perfect iron, powerfully attracted by the magnet; the one $\frac{5}{16}$ of an inch in length and one line in breadth, with craggy points; the other, about half the dimensions.

I asked her mother, who was present, if she were sure they were not in the bed pan before it was used? She replied, she had paid particular attention to that circumstance, and was certain there was nothing in it immediately before it was used. I then inquired of the patient if she had ever taken any medicine of which iron was an ingredient? She answered, "In March, 1811, (18 months ago) my Physician gave me a paper, containing what he called a preparation of steel to be infused in a bottle of wine, of which I was directed to take several times a day. On a certain evening, I received a dose at the hand of a young lady, which, upon swallowing seemed so coarse, and gave such uneasiness as caused me to exclaim, "What have you given me? dregs and all? I believe it will kill me."

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* The frequent necessity for the use of the catheter, as well as many anomalous symptoms, attending this case, and also some unusual effects of remedies, will probably appear to others as they did to me, somewhat extraordinary. I can only explain them by the extreme irritability of the system induced by a cause soon to be developed.

It ought not to pass unnoticed, that an aged and respectable aunt, present when this occurred, distinctly remembers these expressions. Her mother now subjoins: "Mary, I now remember saying to you, "I dont know but your steel medicine has caused your hiccups: for they did not begin till after you had taken of it."

It was now an interesting question, whether the injury sustained by the urinary organs would heal by the first intention, or whether ulceration would follow; and if so, what influence the injury or destruction of one kidney would have on the functions of the other. We therefore noted the circumstances attending the discharge with particluar circumspection. For a week, there was a regular discharge two or three times in twenty-four hours without the catheter, though not without pain. The pain and soreness was then so much increased as to occasion a suppression, rendering that instrument indispensable. Soon after, purulent matter was seen to float distinctly in the urine. The discharge was not constantly purulent; but alternately purulent and limpid; in quantity from lb. ss. to lb. i.

Sept. 15. The excretion began to recur more rarely; the period gradually increasing in length till on Nov. 4th, it had been suspended 16 days and 11 hours; after which the space between the excretions was from 14 to 11 days till March 1813, after which it gradually shortened through the succeeding summer, when it resumed its natural frequency.

From Oct. 13 to March 10, a period of 21 weeks, there were only 12 urinary evacuations, amounting in all to 13 lb. ʒ6. These 12 discharges (2 or 3 of them excepted) containing each from 12 to 85 small scoriæ of iron, resembling such as are chipped off when cutting the thread of a screw, being curved in form of a crescent, sharply pointed at each end like a surgeon's needle: in length, if straightened, from 1 to 3 lines.* All the iron discharged has been subjected to the test of the magnet, and perfectly obeyed its power.

Previous to each evacuation, since the periods have been so long, she has had 1 or 2 days notice, by an increase of sickness, and of pain, beginning in the region of the left kidney and thence proceeding to the pelvis. She repeatedly observed at these times that she felt as if full of cambrick needles.

In order to ascertain whether the secretion went slowly on, throughout those periods, or did not commence till towards their close, I introduced the catheter 2 days before the expected excretion, and 12 days subsequent to the last, and obtained only ½ a table spoonful of urine. Since the intervals became so long, the urine has been purulent once only.

She has been subject to paroxysms of nausea and vomiting, from the time she swallowed the iron in March 1811; but soon after her nephritic symptoms commenced, they became more frequent: assuming a diurnal revolution, they commenced earlier, and continued later each days, till at length she was in the constant act of vomiting most of the 24 hours, for many days in succession.

This sickness is of a character somewhat peculiar: it manifestly differs from an idiopathic affection of the stomach, such as cholera which for the most part rapidly exhausts the patient.

It seems more anamolous to certain sympathetic affections of that organ; for instance, that which attends uterine gestation. The efforts of the stomach though extremely violent, are almost fruitless: nothing being brought up but a little bloody mucous; or what has been recently swallowed. The patient feels confident that it is excited by the nephritic affection; and it evidently quadrates with the pain in her loins, which is a constant, though fluctuating symptom.

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* The scoriæ first discovered, amounted to the number of 12, the largest of which I found would pass with ease through the perforations of the catheter; but lest there should be some larger remaining still to come away, I had the perforations enlarged to 3 lines in length, and one in breadth, which proved amply sufficient to admit the largest that ever passed afterwards.

At the time when this symptom was at its acme, it was the apparent cause of a distressing affection of the œsophagus and larynx, manifested by a very difficult deglutition and a hoarseness resembling croup. At this time I was called out of bed by her nurse, who seemed to think her life and sufferings were drawing to a close. I found that for some time she had been unable to swallow, and that the attempt occasioned a struggle and agony which impressed on my mind the idea of Hydrophobia; though a circumstance soon to be related, destroyed the seeming analogy. Her pulse were almost imperceptible, her extremities pale and cold, her countenance much contracted and pale, and the powers of life apparently retiring to the citadel.

I asked her if she could think of nothing which she could possibly swallow? In a feeble whisper she replied, "cold water." Some water very cold was given her, which she swallowed in small quantities; and though with much difficulty it was extremely grateful. This article, with tamarind water, citric acid, and pickles, constituted her bill of fare for several days; after which she began to use food, if such it might be called, a little more nutritious: Such as raw apples, very sour, figs, raisins, almonds, &c.; but her choice of nourishment continued remarkably fastidious for a long time, and the slightest deviation from what was congenial to the dictates of her capricious appetite, was sure to exasperate the vomiting excessively.

To relieve her sickness, the whole tribe of Antiemetics were found totally ineffectual, if we except a mixture of ten parts, Elix. Prop. and one each of Ol. Absinth and Ol. Menth. Vulg. After she began to take cold water, her stomach was perfectly quiet for twelve hours; when unfortunately the music of a military company passing by, occasioned such violent vomiting as she could hardly survive.

From this time, however, it gradually abated, and though daily aggravated by various causes, has never returned with equal constancy or severity.

For the affection of the larynx, she took a pill of Calom. and Opium, which seemed to quiet the irritation generally, and to diffuse and equalize the excitement. In a few days a gentle ptyalism was excited; but notwithstanding this inconvenience, she was so sensible of their beneficial effect, as to desire their continued use.

Recovering gradually from these distressing symptoms, she was able by the middle of December to be carried on a bed in a close carriage to her fathers house, a distance of three miles, though it cost her twenty-four hours constant retching and much pain in the lumbar region, she soon appeared as well as before.

She was from this time convalescent with a few occasional interruptions ; and she now enjoys tolerable health, except that her stomach rejects animal food and amenorrhæa still continues.

OBSERVATIONS.

The features of this case are so strongly marked with novelty, as to challenge, perhaps, a precedent in the annals of medicine. The improbability of another case occurring, or a nature at all analogous, will probably in the opinion of many, render this history, and any remarks, much more pertinent than I shall be able to offer, more curious than useful. I will, however, notwithstanding the obscurity of its circumstances ; hazard a few reflections on its exciting cause and various phænomena.

The introduction of extraneous substances into the human system, is an occurrence by means uncommon, as the records of medicine and frequent observation sufficiently evince. But that such a multitude of distinct, small ferruginous bodies as above discribed, should all find their way by the same selfcreated passage from the œsophagus to the left ureter, and from thence be voided at such distant periods of time, seems to me not very easy to comprehend.

Did they when swallowed pass immediately into the stomach ? Or where they detained in the œsophagus ?

This latter hypothesis may at first view appear incredible : but I am not sure that the other, will, on examination, be found less embarrassed. As one of the two must be the fact, we are reduced to a choice of difficulties which merit a respective discussion.

If they passed directly into the stomach, we might expect they would subside collectively to the most depending part ; which part might possibly be in contact with the left kidney. Here, if nothing should disturb their repose, they might possibly rest till they eroded the coats of this viscus, which, forming an adhesion with the kidney, might allow them to penetrate its substance, and finally to reach the ureter.

But they undoubtedly excited the hiccups. Should we not then more naturally expect to find the cause of this symptom exerting its action on the cardia, or more probably still on the diaphragm itself? That these substances should attach themselves to the cardia, seems to me less probable than to the œsophagus. When they were swallowed, the patient was in a recumbent posture; she complained of their sticking in her throat, and tried various expedients to promote their passage into the stomach: Might they not remain in some part of the œsophagus and become a nucleus to a body of coagulated lymph which would sheath them over, and confine them till they perforated the œsophagus and fell on the diaphragm, and at length worked their way into the left kidney, which lies higher and nearer the diaphragm than the right?

But enough of hypothesis, lest I continue to darken council by words without knowledge. They by some rout or other, passed through the left kidney into the ureter, where they announced their arrival the sixth day after the cessation of the hiccups, placing us in doubt and uncertainty whether this symptom yielded to medical aid, or to the transition of the exciting cause, from parts more, to parts less susceptible of excitement to this morbid action.

My grateful acknowledgments are due on this occasion to the venerable Dr. Holyoke, and most of the respectable Physicians of this and the neighbouring towns, for their friendly attention to this extraordinary case.

Salem, April 1st, 1814.

ON THE PHOSPHATE OF IRON.

IN the last Number of this Journal were published several cases in which the Phosphate, of Iron had been exhibited by Dr. CALEB MILLER, of Bristol, with considerable benefit. Since that publication, the Editors have received from Dr. M. a number of communications on the use of the same medicine, in a variety of cases, which appear to have been equally benefitted by its use. He informs them that farther experience has corroborated his previous opinions of its efficacy, in cases similar to those already published, and his opinions have been also confirmed by the experience of his professional brethren in his neighborhood. A part of the cases are as follows.

A CASE OF INTERMITTENT FEVER.

Bristol, Sept. 30, 1814.

A GENTLEMAN residing at New-York for two months, was taken with an Intermittent Fever, which continued for about fourteen days, with a regular Paroxysm every day.

He applied for Medical aid, and was treated with the bark and wine, but it not having the intended effect, after being continued some days, it was recommended to him try Fowlers Mineral Solution, which he did, with the happiest effects; for it suspended the Paroxysm on the first day, and from that time he continued well till he came to Bristol, which was about one month after.

He was taken again with the Paroxysms, after residing here about ten days, and suffered them tocontinue three days; then applied to me. I being out of town myself, a pupil of mine ordered an Emetic and Cathartic; the next day followed with the bark and wine, which were pursued for three days.

On coming into town, I found this gentlemen somewhat debilitated with the loss of appetite, &c. I ordered him the Phosphate of Iron, a drachm, three times a day, which suspended the Paroxysms. He continued well till the 25th of Oct. when the Paroxym again returned. I ordered the Phosphate of Iron as before with the same effects, and he remained well till the 1st of November, when he was again taken. Not being satisfied as to the nature of the complaint, it yielded so readily to the Medicine, I thought proper to omit the Medicine for a few days so as to ascertain the nature of the disease. I found the Paroxysm came on regularly every day, and being satisfied as to the nature of the complaint, I again ordered the Phosphate of Iron as before, but the effects were not the same, for it only only mitigated the Paroxysm. On the 5th of Nov. I ordered him one drachm and a half of the Phosphate of Iron to be taken once in two hours. It mitigated the Paroxysm as before. On the 6th, I ordered two drachms to be taken as before, and with the same advantage. 7th, Continued the same as yesterday with greater advantage. 8th, Ordered the Medicine as before. 9th, The Medicine the same with a drachm of the Phosphoric acid, to be taken about one hour before the expected Paroxysm. He has not had any fits to this date, (November 20.) His appetite became considerably impaired, and I think it was partly occasioned, by the large doses of the Iron. I should not recommend it in so large doses in another case like this.

This is the only case of the kind, in which I have had an opportunity of trying it; I only mention this that others may have an opportunity of seeing what it did in one case; but perhaps the same happy effects may not be experienced in another; experience only will determine.

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In another communication Dr. Miller states the success with which the Phosphate of Iron was exhibited in a case of Dyspepsia. In this as in most of his cases it was given in doses of a drachm three times a day. In the same letter are two cases which seem to shew that among its other various effects, that of an emenagogue is to be numbered. In one case its action on the utenus was so great that abortion was the consequence. Should this article therefore come into much use, the circumstances under which it might appear to be indicated are to be cautiously inquired into, and this emanagogue property which it possesses in common with some other preparations of Iron, to be had in view. Dr. John Winslow, of Swansey, in a letter to Dr. Miller, remarks that the emenagogue property of this remedy were very striking in a case of Amenorrhæa in his practice, and details the particulars, which confirm the experience of Dr. Miller.

A case of Scrophulous swelling on the neck.

April 16, 1814.

A married woman, aged 23, had a tumor a little below the ear, about half the size of a hen's egg, but differing in shape, being rather wider and longer. It made its first appearance about two years ago. It had given her a great deal of trouble and many things had been done, but all to very little purpose. She had, together with this, a pain and numbness in one half of her head and face, so that she was not able, at any time, to sit at the window, or in any place where there was a current of air. She observed that this side of her face was cold; at this time she was much debilitated, and was scarcely able to ascend the stairs. I thought proper to order the Phosphate of Iron, 35 grs. three times a day. Under the use of this, her general health began to improve, and in four weeks she enjoyed much better health generally, and could ascend the stairs actively. The tumor in the mean time I thought rather diminished, the pain and numbness in her face were much less. July 20, she informed me that her head was perfectly well, and would bear a current of air as well as ever. The tumor in the mean time rather diminished, I ordered a continuation of the medicine. She continued to follow my directions till the 26th of September, when the tumor had wholly subsided, and now says she enjoys good health.

A case of chronic Rheumatism is reported, which appears to have been relieved by the exhibition of the Phosphate of Iron. Also one of enlarged glands in the neck which is also stated to have been cured by the same means. This last case is contained in a letter from Dr. Barrows of Bristol, to Dr. Miller. The Medicine was given in doses of a drachm, three times a day. In this case as in some others communicated on the same subject, it does not appear that any other remedies had been premised, or that the cases were very severe. We have not, of course, the result of comparative practice, and the superiority of this medicine over others in common use, does not appear. It is however proper to remark that such comparison in some other cases was instituted, and the result in many of them is stated to be in favor of the new remedy.

DEDHAM, AUGUST 15, 1814.

DEAR SIR,

I WAS desired to visit a child five years of age, a daughter of a respectable gentleman in the vicinity of Boston.

She had been afflicted for two years past, with a troublesome Diarrhœa and a very inveterate eruption on the skin, which at times penetrated into the adipose membrane, so that it formed very troublesome ulcers.

She appeared to be alternately affected with the eruption on the skin and Diarrhœa. When the bowels were quiet, the eruption would appear on the skin, and when the eruption was on the skin, the bowels became quiet.

Many articles of the Materia Medica had been made use of; some of them appeared to mitigate the complaints, but it was but for a short time that any of them could be depended upon.

Being disappointed in these articles, I had recourse to the Phosphate of Iron. My idea in giving this, arose from the beneficial effects I had just seen produced in a case similar to this, but less in degree. I ordered it in doses of a about 20 grs. three times a day.

17th. The medicine had been given according to directions, had agreed very well with the stomach and bowels.

19th. The medicine continued to sit well on the stomach, and bowels not so irritable.

21st. The health considerably improved, bowels not so irritable, and the ulcers put on a better appearance. Ordered the medi cine to be continued in doses that should sit well on the stomach and bowels.

I now went into the country, and was absent about three weeks, and on my return, found the medicine had been given in much larger doses; my patient's bowels were now in good order, the eruptions had disappeared, and the ulcers were healed.

Nov. 26. I saw my patient, she had become quite fleshy, no complaint of bowels, has had no appearance of the eruption; some of the scars still remain, but the child enjoys health in perfection.

I am respectfully,

Your sincere friend,

JESSE WHEATON.

Dr. Caleb Miller.

Following this is a letter from Dr. Miller, containing an account of a case of œdema of the left leg, which took place soon after delivery, and which became chronic. The Phosphate of Iron was given to this woman, after having made use of a roller which had produced some good effects. The Phosphate was given in doses of a drachm three times a day, and in a few weeks the swelling disappeared.

In another letter, Dr. Miller communicates a case of incontinence of urine in a lad of 16 years of age, which appears to have happened in consequence of a fall from a considerable height. The urine was evacuated in the day time every hour, and often in the night its quantity was considerably increased. He took a drachm of the Phosphate of Iron three times a day, with relief; and in a short time, with a perfect cure. There had been for three years previous to the accident, an unusual disposition to evacuate the bladder, but which had not been attended with any marks of ill health.

Cases of Diabetes are referred to, which are said to have been benefitted by this medicine.

CASES OF GUN-SHOT WOUNDS.

[From the New Medical and Physical Journal.]

1. *Case in which the Ball passed through the Bladder and Rectum without proving fatal.*

By M. Gualtier, Surgeon, &c. to the Hospital of the Imperial Guard.

WOUNDS of the abdominal viscera are, for the most part mortal, either from the effusion of fæces and urine into the general cavity, or from the gangrene which generally takes place. Sometimes, however, Nature will succeed in curing the most desperate cases, and evince resourses which could hardly be credited by those who are not witnesses to them. Of this, the following is an example, the principal circumstances of which I shall relate, although not full acquainted with the detail of minute particulars.

An infantry soldier of a regiment of the Line, was, in the beginning of July, 1808, in the expedition to Valentia, wounded by a musket ball, which struck, and partially fractured the os pubis, and came out at the posterior surface of the pelvis, near the right lateral part of the os sacrum, just above its articulation with the coccyx. Fragments of bone occasionally came away from this wound, which, together with its being situated in the substance of the sacrum, leaves no room to doubt a part of this bone being fractured. Besides, both the urine and fæces pass away by the wound behind, while the urine alone flowed through the anterior wound. The effusion of the fluid contained in any cavity, is the only true pathognomonic sign we have of the containing viscus having been penetrated by some instrument; so, that we may be certain here of both the bladder and rectum having been wounded, from the escape of their natural contents.

I shall not enlarge upon the circumstances of the patient not falling a victim to this severe wound; whether effusion of fœcal matters took place into the abdomen, or how the patient survived the inflamation and gangrene which would immediately follow such effusion; nor shall I advert to the miserable condition in which the patient was left, almost destitute of surgical assistance, placed upon a wretched cart, without bread to appease his hunger or water to allay his thirst, in the midst of the sands of Valentia, suffering from a burning sun during the day, and the cold air during the night. I merely relate the fact. The soldier arrived at Madrid, at the end of July, and was left there when we retreated in the following month. I found him there in December nearly six months after the accident, enjoying perfect health; the posterior wound was completely cicatriced, and the urine and fæcal matters had not passed through it for a considerable time previously; the anterior wound was reduced to a small fistulous opening, situated at the bottom of a deep depression immediately above the pubis. There appeared a fistulous opening below the pubis, resulting from the long continued presence of a trocar, with which the hypogastrium had been punctured; a few drops of urine escaped from this opening, when the bladder was very full. The patient for some time experienced great weakness, and almost a paralysis of the right lower extremity; when I saw him, no traces of these remained.

2. *Case of a Gun-Shot wound, followed by a total loss of the Scrotum.*

By M. Guillon, Surgeon of the millitary Hospital at Talavera.

A soldier of the Imperial Central Army, in Spain, was struck by a musket ball, which entered his left side near the top of the coccyx, about half an inch from the verge of the anus: the ball could not be discovered by the most dilligent search. The wounded man at first felt no uneasiness, he could as usual pass the urine and fæces in a natural way. In about twenty-four hours the scrotum became very painful, it swelled and inflamed to such a degree, that gangrene soon followed; the progress of this was rapid and extensive, speedily destroying the whole covering of the testicles. The ball discovered itself, and came away through the gangrened parts; the gangrenous affection now diminished, the sloughs fell off, and left the tesicles completely exposed. Suppuration took place, granulations sprung up in great abundance; and the intergumients of the anterior part of the abdomen, those of the internal parts of thighs, and those which cover the groins, all lengthened out in a surprising manner to form a new covering for the testicles, which receded upwards a little, so as to occupy less space.

Thus in the course of six weeks, and as it were by a prodigy, this great devastation was repaired. Some filaments were produced, which adhering short, were at first troublesome and painful; but they gradually lengthened and the movements became easy, and this soldier, notwithstanding all the danger he had been in, was restored to his regiment in abont two months. The circumstances most worthy of observation in this short case, are, the track the ball took so as to wound neither the rectum, the bladder, the spermatic cords, nor the testicles; the facility with which the ball came away after the integuments became gangrenous; the promptitude with which this ceased when the foreign body was expelled, and expecially the ingenious method Nature took to cover and preserve from the contact of air, the very important organs of generation. I do not speak of the treatment; because, in fact, there was not any effectual one could be adopted. When the ball could not be found and extracted, the only real indication was not to counteract Nature. In the present case, as in many others, the vital power and the vigor of the parts effect every thing; it is our part to contemplate the wonderful phænomena produced by Nature, without opposing their progress by our ignorant and officious interference.

REVIEW.

Medico-Chirurgical Transactions, published by the Medical and Chirurical Society, of London. Volumes third and fourth London: Printed for Longman, Hurst, Rees, Orme, and Brown, Paternoster Row. 1812, 1813.

SOME account of the first and second volumes of the Medico-Chirurgical Transactions has appeared in the former numbers of this Journal, and we have much satisfaction in being now able to lay before our readers, an abstract of the contents of the third and fourth. This work combines so much information on the various subjects connected with medicine, that is without doubt one of the most important and interesting publications, that has been presented to the faculty, for many years. The high reputation of the men who compose this society, excited the greatest expectations; and these have been most fully satisfied by their periodical labours. Should any turn over these volumes with the hope of finding only brilliant theories and fanciful speculations, they will undoubtedly leave them with disappointment. The publishers present clear statements of facts, and draw from them such conclusions, as their experience will warrant.

The third volume contains twenty-one papers; some of which are on medical and chemical subjects, and others, on those connected with Anatomy and Surgery. The volume opens with "*Facts and Observations respecting Intermittent Fevers and the exhalations that occasion them,*" by Sir GILBERT BLANE, Bart. The author was sent by the British Government, to Walcheren, in the autumn of 1809, to investigate the nature and causes of the sickness that afflicted the soldiers who were engaged in the calamitous expedition to that island. Upon his arrival he found that one half of the army was sick or convalescent, and that this number was increasing. "Nor was this great sickness," says he, "imputable to any thing unfavourable in the weather at this season, in comparison of former years. On the contrary, the native inhabitants affirmed, that they were then less sickly than usual at the same season of the year, and they accounted for this, from the uncommon quantity of rain that had fallen the last two months; for they consider it as fully established by observation, that the most sickly years are those, in which there had been great drought and heat in the latter end of summer and the early part of autumn; owing, probably, to the increased exhalations and the more concentrated foulness of the stagnant water produced by these causes." He found upon inquiry, that the army had suffered "neither from the scantiness nor bad quality of provisions, nor from want of proper accomodation, nor from hardship or fatigue," but that the sickness was imputable only to the contamination of the air by the deleterious exhalations from the soil. These exhalations had usually produced an endemic fever in the autumn, which affected strangers with the most severity. Having thus satisfied himself that the causes were in a great measure beyond human controul, he recommended that as many troops as possible, should be removed to Great Britain. But as this could not be done immediately, he was induced from two facts with which he became acquainted, to employ artificial means to counteract the influence of natural causes. "One was, that those belonging to the upper orders of society in Walcheren were always less affected with endemic fever than the lower inhabitants; the other, that the British officers suffered less in this campaign, than the private men." As this was undoubtedly owing to their superior accomodations, the author recommended the "use of stoves in the barracks as well as the hospitals" and also, that the men should be supplied with hot breakfasts. He considered both of these important, as the inhabitants remarked that those who occupied the upper stories were less liable to infection than those who lived on the ground floor: and it was observed by the French general Mouret, who commanded during the siege, that a small quantity of spirit in the morning, and the addition of pepper to their broth and other articles of food, tended to preserve the health of the soldiers.

The author remarks, that Typhus and Dysentery which are most usual among soldiers at this season of the year, in the low countries, were by no means prevalent in the British army, but that the soldiers enjoyed remarkable health during the first month of service. He considers this to be owing in part to mental causes, "to the hope of victory and success which is favorable to health," and partly to the fact that some time is requisite for the operation of miasmata on the human body.

On the 25th of August, 1809, there were 41,642 rank and file, and non-commissioned officers, and 1879 commissioned officers, and only 14 died of diseases in the first Calendar month of service. Between the 21st of August, and the 18th of November, however, 26,846 of this number, including relapses, were sent to the hospital; and between the 21st of August, and the 16th of December, 12,863 sick were sent to England. It was found, that of the British troops, the natives of mountainous countries, and dry soils, were more frequently affected than the natives of flat and moist districts.

If there is any thing we have to regret with regard to this paper, it is that the author has not given a more full and detailed account of the symptoms and treatment of this disease. The mortality that attended it was so alarming, that it excited the attention of the faculty in most parts of this country, as well as of Europe; and it might have been hoped that the present work, would have given some more interesting information on the subject than we before possessed. We will quote, however, from the author, a few remarks concerning the disease and curative method. "It is evident that the severity of the symptoms in the Zealand fever added greatly to the difficulty of the cure, and there could be no opportunity of employing Peruvian bark or other specific remedies, till its violence had abated, and the redundant bile had been carried off. The treatment of this acute state, consisted chiefly in giving such remedies as purged freely; and in selecting them, the preference was due to those which acted most readily on the liver and the bile, such as calomel, those which were least heating, as the neutral salts; and such as were best borne by the stomach, which in a great many cases was extremely irritable. In the course of the general inspection, in which my duty consisted at this time, I had not myself an opportunity of directing and watching the practical details of individual cases; but I had considerable experience in this way, in any attendance on officers in England, who either brought the complaint with them, or were seized on the passage, or after their return to this country." The author makes some interesting remarks on the subject of hepatic affections, and states it as his belief that derangements of the liver in the autumn, predispose the patient to intermittents in the low countries. He does not believe that pure humidity is noxious, though "it may prove pernicious as a vehicle of unwholesome volatile matter." He had an opportunity of observing the extent to which the noxious exhalations were carried, and he states that the crews of the ships that lay within 6000 feet of the shore, were in no instance affected. The paper concludes with "remarks on the comparative health and population of England and Wales, at different periods."

The second paper is communicated by Thomas Chevalier, Esq. It is a history of a *dropsy of the ovarium*, from which no less than seventeen gallons of fluid were drawn, at four operations by punctures made in the linea alba. Upon examining the body after death, it was found that nearly the whole cavity of the abdomen was occupied by two cysts formed in the left ovarium, which were closely connected together, and attached laterally and anteriorly to the parietes of the abdomen. The upper cyst contained about two quarts of a brown, glairy fluid, and the inferior, three gallons of purulent fluid, which was no doubt the effect of the inflammation that produced the death of the patient. The general health had been good during the whole progress of the disease.

"*A case of difficult parturition*," forms the subject of the third communication. It is furnished by Dr. Merriman, who attended the patient, and found that the difficulty arose from the right ovarium being enlarged and laying between the vagina and rectum. He recommended in those cases where the tumor evidently contains a fluid and obstructs the passage, that a puncture should be made in it, and if it is hard; he suggests the propriety of making an incision, so as to evacuate it, and give a passage to the head of the child.

The two next pages are from John Parkinson, Esq. and contain a case of diseased appendix vermiformis, which terminated in the death of the patient.

The fourth paper contains a case of "*diseased testicle*," furnished by James Earle, Esq. with an appendix by William Lawrence, Esq. The patient was a child of twenty-one months old, when the author saw it; the testicle had grown to such a size as to reach to the inner condyle of the femur when unsupported; and medicines having been tried without producing any alteration in it, it was extirpated. After the operation, the general health improved, and the wound gradually cicatrized. These favorable appearances, however, continued but a few months; the disease seemed to return with more violence, and the brain was evidently the part now affected. The author had an opportunity of seeing the patient but a few times after the recurrence of the disease before he died. On examining the body, four tumors were found in the brain. The largest was of the size of an orange, the smallest as big as a chesnut. They were of a very firm consistence, of a dusky red colour with streaks of white interspersed through the substance. They were so closely connected with the substance of the brain, that they were with difficulty preserved in their situation. Many large tubercles were found in the lungs. The mesenteric glands were enlarged, though they exhibited no marks of disease when they were cut into. The author thinks that this disease is of the same nature as those known by the names of Fungus Hæmatodes and Medullary Sarcoma; he objects however to both of these terms, and denies that they have any thing in them indicative of the disease. The appendix contains four cases which terminated fatally, and in all of which, death seems to have been produced by a disease similar to that above described, though it affected different parts, such as the axillary glands, uterus, lungs, &c. A very neat plate, representing the morbid appearances in the brain, accompanies this paper.

A description of an improved method of *tying the tonsils*, is the subject of the fifth article, which is from Mr. Chevalier. The improvement consists, "first, in the mode of passing the ligature; and, secondly, in the mode of securing it when passed. 1st. In order to pass the ligature, I use, says he, a flat spear-pointed hook, fixed in a handle, the broadest part of which is about two lines. This hook is passed behind the diseased tonsil, and its point is then pushed forward, so as to perforate it, through the middle of its base. It is then to be withdrawn. An edged probe, very much curved, and armed with a long double ligature, may then readily be passed through the perforation, and brought out at the mouth, and the ligature divided, so that one portion may be tied round the upper half of the tonsil, and the other round the lower. 2. In order to effect this, I have availed myself of a noose, much in use among packers; which, though it might readily be loosened, if a mechanical power were acting against it, remains under other circumstances perfectly fast; and is fully sufficient for the purpose in this, and other analogous cases, where a ligature is required. A single knot being first made upon one end of the thread, the end so knotted, is to be bought forward upon the other, and to make a single noose upon itself including the other, and to be drawn tight upon it, close to the first knot: the free end of the thread is then to be passed though the ring of the instrument. This end of the thread being then held firm, and the ring pushed forward upon the knot, the loop now formed, may be readily tightened, so as completely to strangulate the diseased part; and in the same manner it may be tightened from day to day, till the part is entirely detached." After the operation, a good deal of inflammation comes on, in the mouth, fauces and neighboring glands, for which small doses of laudanum and antimonial wine may be administered. Poppy fomentations applied externally have been found beneficial. The inflammation begins to subside usually on the third day, and the tumor is detached on the fourth or fifth, when the sore readily heals. Six or seven inches of the ligature must be brought out at the mouth, and fastened by adhesive plaster to some part of the neck. A plate of the instruments and ligature is given at the end of the volume.

Communications No. vi. and xix. are from J. R. Farre, M. D The first consists of cases of *Cynanche Laryngea*, and the second of remarks on the distinction between that disease, and Cynanche Trachealis. Two cases only are recorded in the first paper, both of which terminated fatally. In the first case, there was every symptom of active inflammation; the eyes protruded, respiration difficult, the uvula large, œdematous and speckled, the pulse laborious, and frequent. Emetics, cathartics and venesection were employed without effect. The body was examined after death. The pharynx and œsophagus being slit down posteriorly, the mucous membrane investing the epiglottis, rima glottidis and the posterior part of the larynx was found to be œdematous to so great a degree, as to make it evident that suffocation had been produced by stricture of the glottis. In the second case in which the symptoms and treatment were similar to those of the last, Mr. Astley Cooper performed the operation of bronchotomy, "by dividing laterally the ligament which connects the thyroid with the cricoid cartilage." The dyspnœa was relieved by the operation and the breathing continued through the natural and artificial apertures. In a few hours it ceased by the natural passage, the patient soon began to sink, and shortly after expired. The appearances after death differed from those of the last case, inasmuch as the inflammation had proceeded farther. The Pharynx was much inflamed and covered with coagulated lymph about the epiglottis. The author prefers the name of Cynanche Laryngea to that of Cynanche Pharyngea, for though the Pharynx is the seat of inflammation, the disease, "proves fatal by constricting or actually closing the glottis, and constitutes precisely that case which in its ultimate degree, imperiously demands the operation of bronchotory." In the second paper the author gives the marks which distinguish Cynanche Laryngea from Cynanche Trachealis, which he illustrates by some interesting cases. We shall quote what he says concerning the diagnostic symptoms: "In all the cases of Cynanche Laryngea, the mucous membrane investing the epiglottis, glottis, and chiefly the external and posterior surface of the larynx, was the seat of the inflammation, by which the rima glottidis was so much narrowed, that the vital functions were actually extinguished by the stricture. It is true that the tonsils, the arch of the soft palate, the uvula, the tongue, the internal membrane of the larynx and trachea, all, or several of them were inflamed; but this was only an extension of the action to one common membrane which invests the whole, whilst it was most intense at the mouth of the larynx. The difficulty of the diagnosis does not in fact lie between this inflammation and the cynanche tonsillaris, but between it and the cynanche trachealis. Croup very nearly approaches the cynanche laryngea, but the difference between the seats of the inflammation will appear by comparing the symptoms and morbid appearances which belong to each disease. In the cynanche laryngea the symptoms are, uneasy sensations in the larynx, difficult and painful deglutition, partial swelling in the fauces, a supervening and perpetually increasing difficulty of breathing, inflammatory fever. In the cynanche trachealis there is a difficulty of breathing, without any swelling of the fauces, or painful deglutition, the expirations, especially in coughing, are very shrill, the fever is inflammatory. In both, the voice is changed, and, in extreme cases, is suppressed; the termination is by suffocation.

The following are the morbid appearances: in the former, the mucous membrane investing the epiglottis and margin of the glattis is inflamed; serum is effused under it, or coagulable lymph on its external surface, by which the rima glottidis is narrowed or actually closed. In the latter, the mucous membrane of the larynx and trachea is inflamed, and a layer of coagulated lymph is formed on its internal surface, from the extremity of the epiglottis to an indefinite extent within the trachea, by which the tube itself is narrowed, or actually closed. A puriform fluid, instead of mucous, is found in the trachea and bronchiæ. These characters chiefly apply to simple cases of both species, for as they differ in their seats, but not in their nature, it is possible for both forms to be combined in the same patient, and it were an easy matter to prove that a still greater exertion of inflammation than this does actually take place in the organs."

The seventh article consists of a case of *Anæsthesia*, commudicated by John Yelloly, M. D. It "exhibits an example of the loss of sensation in the upper and lower extremities, independent of paralysis." The following are the principal circumstances of the case. The feet half way up the leg, and the hands up to the wrist were perfectly insensible to every species of injury. The patient accordingly put one of his legs in some boiling water, and he discovered its high temperature, in no other way, than by the vesications it produced. A couching needle was thrust more than half an inch in depth into the ball of his thumb, so as to touch the bone, and it produced not the least degree of pain or sensation. The wound healed readily. The powers of motion existed in the hands and feet, the functions were natural, and the pulse regular and moderate." Various remedies were tried, and all without effect. Among these, warm bath, electricity, galvinism, tonics of different kinds, and mercury were used, and all proved equally unavailing. The author mentions that he has met with but two cases that were very similar to the one he relates. The first is to be found in the Medical Repository of New York, and is communicated in a letter from Dr. Samuel Brown, of Lexington, Kentucky. The other is in the "Philosophic Zoologique," by Lamarck. In closing this communication, the author remarks that "the existence of muscular power, and the faculty of directing its exercise by the will, where the nerves have entirely lost that sensibility which is always regarded as necessary to the conveyance of volition from the sensorium, are circumstances apparently irreconcileable with any knowledge which we at present possess, of the mechanism by which the will acts in the production of voluntary motion."

Mr. Chevalier has furnished the eighth article, in which he relates three cases of *extravasation* of blood and serum within the *Theca Vertebralis.* He considers them anlogous to Apoplexy, and recommends depletion, before constitutional debility takes place.

Dr. Boctock, of Liverpool, is the author of the ninth and eleventh communication. The first is the case of *Diabetes Insipidus,* which he considers to be a rare disease and totally different from a mere increased secretion of urine which arises from derangement of the kidnies only. The author, who is well known as an accurate experimenter, ascertained that there was no saccharine matter in the urine. We regret that our limits will not permit us to give a full account of this paper, which is highly creditable to its distinguished author. We will only observe, that he considers the three following to be "pathognomonic symptoms of diabetes, viz. the increased appetite for food, the suppression of the cutaneous discharge, and especially, the evacuation from the urinary organs of a larger quantity than ordinary of animal matter. This latter circumstance is essentially different from the mere increase of the quantity of water, a morbid state which has often been confounded with diabetes, but from which I regard it as totally dissimilar." All these symptoms were found in the case which he has published, and the patient was cured by the administration of tonic remedies and close attention to the general health.

The second paper contains experiments on the bark of the *Coccoloba Uvifera,* with eight different reagents, and also on the kino. From these the author concludes that "the extract of the Coccoloba is a substance of the same nature with kino used in medicine ; but that it differs from it so far as to show that they are not derived from the same plant." The experiments in both these papers, seem to have been performed with great care, and the communications evince much nice and accurate discrimination.

Dr. Merriman has given several cases of *premature labour,* artificially induced, which form the subject of the tenth paper. In all these cases the patients had been pregnant before, and it had then been ascertained, that the distortion of the pelvis was so great, that it was impossible to deliver them of living children at the full period of gestation. Under such circumstances, the author deemed it proper to bring on prematura labour by puncturing membranes, about fourteen days after the expiration of the seventh month, and out of four cases in which he performed the operation, one was successful. The same thing has been attempted by several others, and he has ascertained, that out of forty seven cases of distorted pelvis, in which premature labour has been thus induced, "at least nineteen children had been born alive and capable of living. If it be considered, that in all these cases, the degree of distortion of the pelvis was so great, as to preclude the possibility of a fœtus at nine months of gestation passing alive, or undiminished, there is, I presume, sufficient proof that the practice is not only admissible, but that it ought to be recommended in such deplorable cases of distorted pelvis." With regard to the safety of the mother, the author thinks that it is not so much endangered as by the attempts that are made to diminish the size of the full-grown fœtal head in utero, or as by the division of the symphysis pubis.

The twelfth contains a case of *Splenitis*, by Robert Bree, M. D. F. R. S. This he considered as a case of secondary Splenitis, in which there was no fever ; the pulse varying from 60 to 70 in a minute, and being quickened only by the fits of pain and dyspnoea. There was great soreness of the left side, occasional nausea and puking, and in the intervals of sickness, an eager deire for food. The patient was cured by active cathartics daily administered.

Charles Bell, Esq. has given in the thirteenth paper, an account of the *muscles of the ureters* that have never been before described. Though he describes them as arising from the mouth of the ureters, and passing down under the inner coat of the bladder, to be inserted into the third lobe of the prostate gland, he thinks however that the prostate gland, should be considered as "the fixed point and their connection with the extremities of the ureters as their insertion." He supposes that these muscles in health, assist in the contraction of the bladder and in closing the mouth of the ureters, and that they are powerful agents in disease. He has given a very clear and interesting account of them, which he has illustrated with three elegant plates

The fourteenth paper contains "the history of a case in which a *calculus* was voided from a tumor in the groin," by T. Copeland, Esq. After the tumor burst, a fœtid discharge came from it, evidently containing fecal matter. The wound became irritable, the granulations were lose and dark coloured, and the patient's general health began to decline. About this time an examination being made with a probe, a hard body was felt, and a calculus of an oval form, half an inch in length, was extracted by enlarging the fistulous orifice. The patient's health soon improved, and the wound gradually healed. The author thinks that the calculus, which was the cause of the abscess, was lodged in the cœcum; and that the fecal matter in these cases, will distinguish them from those in which the calculi are in the gall bladder.

We come next to an elaborate and interesting analysis of the *chemical properties of the animal fluids*, by Professor Berzelius, of Stockholm. This paper is the more interesting as the experiments seem to have been performed with great accuracy, and as results are frequently different from those given by other chemists. We find, for instance, that he differs from Parmentier, Deyeux, Fourcroy and Vauquelin, on the mode in which iron is combined with the colouring matter of the blood, and he denies, as has been supposed by Thenard and others, that there are any resinous particles united with the bile. He first gives an analysis of the blood, and next of the secreted fluids. We should however do injustice to the author, as well as to our readers, if we attempted to give an abstract of this excellent paper. Every part is worthy of an attentive perusal, and is a valuable addition to the science of Animal Chemistry.

The nineteenth paper contains a case of *Fungus Hæmatodes*, by George Longstaff, Esq. The patient, when he applied for advice, had a small tumor on his left shoulder, which was of a fungous nature, extremely painful, and frequently bled. The glands in the axilla were swelled, but free from pain and the skin over them had a natural appearance. The tumor was extirpated; during the operation there was considerable hæmorrhage, and the wound did not heal under five weeks. The swellings in the axilla began gradually to increase and the disease was unabated in its progress. For five months every bad symptom grew worse, and unfavorable ones were constantly appearing. At the expiration of this time, the patient died; during the latter part of his life he was troubled with difficulty of breathing and tenesmus. Upon examining the body, it appeared that the whole system was much affected; disease was found in the lungs, liver, and other parts of the body. "The testicles were perfectly healthy."

"A severe affection of the organs of Respiration," is the subject of the seventeenth paper; communicated by A. P. Wilson, M. D. It is the case of a gentleman who had been for many years affected with dyspnœa, which, though occasionally relieved by emetics and blood-letting, terminated fatally. Upon examining the body, the morbid appearances that were discovered, such as water in the thorax, adhesion of the pericardium, &c. the author thinks were rather the effect than the cause of the disease; and that the symptoms arose from a certain state of the extreme vessels in the lungs, that prevented a complete oxygenation or rather decarbonization of the blood.

In the next paper we find an account of "a new mode of treatment in *chronic Rheumatism*," communicated by Alexander Mariet, M. D. F. R. S. and written by the patient himself. The subject of the case had received, about eighteen years before, a violent sprain on horseback, which occasioned pain in his loins, and in the hip, thigh and leg of the left side, and along the course of the sciatic nerve. For this he had tried various remedies in France, England and Scotland, administered by the most eminent physicians, and found only temporary relief. "Happening about five weeks ago to hear of a case which bore a strong analogy to my own, though the subject was not a human creature, it suggested to me an experiment, which I resolved to try immediately. A celebrated race-horse, had been cured of a disorder which had all the symptoms of rheumatism, by sweating in body clothes, after every other remedy had failed. I therefore clothed myself in a sufficient quantity of flannel, and set out to walk as far and as fast as I could. With the utmost difficulty I proceeded half a mile: and the pain I suffered, contributed not a little to the effect of the exercise in promoting perspiration. I returned home in the profuse sweat, rubbed myself dry before a fire, and went to bed. In about an hour, I got up, found myself very much fatigued, but in other respects not worse. Forty-eight hours after this, I repeated the same kind of exercise, and found that I could walk a mile with as much ease as I had walked half that distance on the first day. Two days afterwards, I took my third walk, proceeding as before, and after it, I had a better night, less interrupted by pain than any I had enjoyed for eighteen months." Every succeeding walk diminished the pain, and after the sixth, there was no symptom left, except a slight weakness of the left leg. Before proceeding to the walks, the patient put on an additional quantity of flannel clothing and girded six yards of flannel on "the chief seat of pain and origin of the sciatic nerve." Upon his return, these were all removed, his body wiped dry, and warm flannel immediately put on. Though he lost flesh during this process, his general health so far from being injured, was rather improved by it. This case is certainly very interesting, and we think that the practice which he adopted, is deserving of a trial in similar cases.

The twentieth communication contains some further remarks on the *nitrat of silver*, as a test of the presence of arsenic, by Dr. Marcet. The object of these, is to answer an objection made by Mr. Silvester, which is, that nitrat of silver cannot be employed to detect arsenic, when muriatic acid is present, as the muriat of silver would be immediately formed, and the yellow precipitate, mentioned by Dr. Marcet, would not appear. To this it is answered that "there cannot be the least doubt, but that whenever the nitrat of silver is added to a solution containing muriatic acid, a precipitate of muriat of silver must be the consequence. But if the nitrat of silver be added in excess, the arsenite of silver is also thrown down by the intervention of ammonia, and a mixed precipitate of luna cornea and arsenite of silver is obtained, which partakes more or less of the yellow colour of the latter, according to the proportion of the two salts. If to this dubious precipitate a few drops of dilute nitric acid be added, the arsenite of silver is instantly dissolved, and the muriat of silver, which is insoluble, immediately resumes its peculiar density and whiteness. If a little ammonia be now added to the clear fluid, the yellow precipitate appears in the most distinct manner and becomes even more characteristic from a comparison with the white precipitate, the appearance of which differs from this in every respect."

The last paper is the history of a case of *remitting ophthalmia*, successfully treated by Opium. The patient was James Currie, M. D. who has communicated the case. He had been for many years subject to attacks of this sort, which had been treated by the usual modes of depletion. These remedies however afforded him but partial relief at the time, and the disease soon recurred. During an attack in 1802, his pain was so excruciating, that he "resolved to obtain ease at all events, by having recourse to opium;" he took two grains in the evening, when his pain was usualy most violent, and to his astonishment no paroxysm came on, and he passed a comfortable night. Frequent recurrence was had to this medicine in increased quantities, before he was perfectly cured; and in a subsequent attack, he found the same advantage from its use. The author thinks that his disease was an epidemic ophthalmia, as the catarrh, which prevailed at the time attacked none who had this affection of the eyes. The case is drawn up very minutely, and contains a number of interesting facts and observations. The reputation of Dr. Currie is such, as to demand the attention of Physicians to any professional remarks he may make.

In the next Number, we shall lay before our readers an account of the Fourth Volume, which was received a short time since.

[We are happy to give insertion to the ingenious Remarks, that follow; being persuaded that a full examination of the subject to which they relate, must be useful. We shall, however, make some repy in our next, for which we have not either time or room in the present number.]

SOME REMARKS ON THE REVIEW OF M. LE GALLOIS' WORK IN THE LAST JOURNAL.

I HESITATE to offer any thing in opposition to a production which exhibits so much ingenuity and research, as the above named Review; to which also I acknowledge myself indebted for a definiteness to many ideas that existed in my mind, as general notions only. But presuming the love of truth to be the predominant motive for the publication, and hoping, by opposing to draw from the same sources additional light, and thus advance the cause of science, I will venture these remarks.

First, it is necessary to settle definitely the meaning of the language used, that our difference may not be merely about words. M. Le Gallois is unfortunate in the choice of words to represent the two principal phenomena, on which rest the proof of his doctrine, and has thus given an opportunity for an objection, which, had he been more accurate, would not have occurred. It is justly objected by the Reviewers, that sensation and voluntary motion are not the principal *criteria* of life; nor do the absence of the powers, which produce these, imply the absence of vitality. But though M. Le Gallois made use of these terms, I think it may be gathered from the general tenor of his language, and from the object of inquiry, that he did not use them in their common acceptation. By sensation he undoubtedly intended to express that property, which Bichat calls organic sensibility, or what may be conveyed by this expression, a susceptibility to the action of stimuli. By voluntary motion, it is evident he did not mean those motions, which are excited by the will, but motions, which are generally called spontaneous, that is, motions, which are the effect of a power resident in the organ itself. Suppose then we adopt for sensation, organic sensibility, and for voluntary motion, spontaneous motion, or what is still better perhaps, *mobility* the property, which is the cause of these motions. To say these, organic sensibility and mobility are the criteria of life would not be "to show, that vegetables are destitute of life; and that man in complete apoplexy is destitute of life; and that parts completely paralized in man are destitute of life;" and yet these words, I believe, convey strictly the meaning of Le Gallois; at least I am willing to rest the question as to the results of his experiments on the strict meaning of these terms. The properties expressed by them, are not indeed the only properities of vitality; but in those parts which are called organic, they afford the principal evidence of life.

I agree with the Review, that Le Gallois' ideas of life do not appear to have been very precise; he seems to have entertained the common notion, that vitality is a single principle, and that the properties of living matter are the effects of this principle; the author of the Review on the contrary, very justly considers the presence of one or more of these properties to constitute, what we understand by life or vitality. But I cannot admit, that, because M. Le Gallois did not have this view of the subject, "the results to be drawn from his experiments will not be the same." A main object of his experiments was, to determine, whether the sensibility of the power of motion of the heart and other organs, are inherent properties, or whether they are communicated by the nervous system. Now whether he conceived these properties to constitute vitality, or thought them to be effects of a single principle, of the presence of which they were the evidence, it appears to me, would not in the least affect the question, as it respects their being inherent, or communicated. The effect of the destruction of the source of nervous influence would be as easily ascertained in the one case, as in the other. If these properties remained in the muscle after this destruction, it would be decisive as to their independence of this influence. If on the other hand, they were suddenly extinguished, it would be fair to infer, that they were dependent on the nervous influence.

With respect to the unity of the vital principle, my sentiments are in uniformity with those of the Review. There is no evidence of the existence of a common mysterious principle, which we are to consider the cause of the vital phenomena; the assumption of it is unphilosophical, and has caused much confusion in this part of physiology. We observe in living beings certain properties, which distinguish the matter composing them from common matter; a combination of these make up our idea of life. No question will then be made between us, as to the seat of a vital principle; we only have to investigate the source of these properties which constitute vitality. It is not however, expedient at this time, to inquire concerning all the vital properties; we are principally interested with two only, organic sensibility, and mobility. The most commonly received doctrine respecting these, was that of Haller, which taught, that they were inherent properties of the muscular fibre. They were designated by this appellation, irritability. M. Le Gallois from the results of some experiments, was led to doubt the correctness of this common opinion, and by prosecuting these experiments he came to the conclusion, that the theory was not founded in truth; that these properties were not inherent, but were communicated by the nervous system. I have repeated many of his experiments, and am convinced, that his general conclusions are correct. The brain is not particularly the source of these properties; the parts composing the head are undoubtedly supplied from it; but the medulla spinalis is the source whence the trunk and the organs which it contains, are furnished with these properties. Destroy the spinal marrow, and the sensibility and the power of motion in the trunk are annihilated; not the least evidence of life remains. This happens too in a way very different from what takes place in the suspension of the agency of any great organ, which is considered vital. Suspend respiration, and the heart will continue its action for some minutes without its aid. Stop the circulation, and the muscles will evince the retention for a long time of their sensibility and mobility. But by the destruction of the medulla spinalis, these are suddenly arrested. With respect to the heart, it loses first the ability to continue the circulation, and shortly the most active stimuli cannot produce in it the slightest motion. Now it is admitted without hesitation that these properties exist independently of the functions of respiration and the circulation; it is equally clear, that they are not indepedent of the function of the medulla spinalis.

This is not to make the medulla spinalis the seat of a vital principle; we do not ascribe it to the production of all the vital properties; we derive two only from that source. These are indeed chief properties, and to us the principal evidence in most cases of the presence of vitality. If a body be not susceptible to the action of stimuli, and if motion does not follow the application of an appropriate kind, we conclude such body to be dead. These however are not sufficient to constitute life in matter; the presence of other properties is also necessary. For instance, the property, by which the seed maintains its integrity for years, and by which it is able to resist the action of many external agents, may be neither its sensibility nor mobility; but the presence of the third property is never so evident, that we can be certain, that the seed is alive, till by experiment the other two are made manifest.

Several functions are enumerated in the Review as essential to active living beings: they are all those of assimilation, and that of respiration. If my view of the function of the medulla spinalis be correct, that also must be added. It may be objected to this, that there are living beings, in which nothing like a nervous system can be discovered. Very true; but it does not follow, because we have not been able to discover the organs, that a similar function is not performed in these beings. We find in them no such organ as the heart, and yet something like a circulation of the fluids takes place. No stomach is there, but digestion and assimilation of foreign matter to the nature of their own substance is accomplished by other means. Can it be said then with certainty, that the function, which is now assigned to the nervous system of the higher animals, that of producing the properties, sensibility and mobility, is not performed in vegetables, because we have not yet been able to discover the means?

It has been assumed, as a principle, apparrently without much reflection, that the properties of living beings are inherent in their composition. But the bodies of living beings are constantly changing; the matter, which now composes the several parts will tomorrow be removed, and new matter will occupy its place. Is this new matter originally endowed with these attributes? We have no evidence of such a fact.* Does it acquire them by any agency independent of that of the being, of which it is about to make a part? Shall we admit the assertion of some of the supporters of the doctrine of Haller, that they are communicated by the fiat of Deity, and thus make a new fiat necessary every time a new particle is added to our bodies? If in neither of these ways this new matter is possessed of the vital properties, they must receive them by some process going on in the body. The Reviewers, who seem to have adopted the opinion of the inherency of some of the vital properties, state, that the exercise of certain functions is necessary to maintain them. How do these functions maintain them? It is not merely by adding new matter to the several organs. Do they not maintain the properties by elaborating them, and communicating them to the newly assimilated matter.

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* I am aware, that some physiologists suppose that matter, which is to be assimilated to living bodies to be different from what they would have us understand by inert matter. They imagine the existence of certain *molecules*, which they call active, and to which they would assign properties, did they know how to name them. These *molecules* are to be assorted and added to the different or-

If it should appear, that the vital properties are the results of the exercise of the functions of living beings, to the direct agency of which of them shall we ascribe the production of the organic sensibility and mobility? Not to that of respiration, nor the circulation; and we have no better reason to refer it to the nutritive process, as that ceases when no longer supplied by the circulation. However indirectly necessary these functions may be, we have seen that they are not indispensable at least to a temporary existence of these properties. If on the contrary we suspend the nervous function, they are immediately extinct. If this were only indirectly necessary to their maintainance, as is the circulation, or were they the effects of any other cause, then continuance ought to be equally apparent, after the destruction of the nervous influence, as after the cessation of the circulation. We without hesitation assign that property, which is the cause of sensation to the brain, as its source, because, as soon as the communication between the brain, and a part enjoying that property is interrupted, the property ceases to be present. For the same reason must we ascribe the properties, organic sensibility and mobility to the medulla spinalis, as their source.

It may be objected, that these properties are possessed by vegetables, and in some animals, in which no nervous system has

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gans of living bodies. This however, appears to be entirely the work of fancy; no proof is offered in behalf of the supposition, and while we have no reason to doubt, that the body possesses sufficient means to convert inert, to the character of living matter, it is unnecessary to consider this supposition.

been found. But this objection amounts to nothing, till it be proved, that no such function is performed in these animals and vegetables. If the vegetable acquires certain properties by means within its own structure, and these properties are the same, as are observed in animals, it is probable, that they are produced by similar means in both. But it is not necessary, that the organs, which perform similar functions should be the same, and as apparent to us in the vegetable as in the animal.

The nervous system in animals is complicated, as it is interested in many phenomena. It is the medium of relation between the mind and the different organs of the body, which are under the control of the will. It is the source of that property, by which sensation is produced, and the medium of relation between the mind and external things. These phenomena we refer to the brain and nerves. The medulla spinalis is another great organ of this system, and accomplishes a very different office. It establishes relations between different organs of the body, and is the source of their powers of motion.

In the vegetable, there is nothing which resembles mind; neither is there sensation; its relations with external things are simple and few; a brain and nerves are therefore unnecessary in its œconomy. The relations between different parts of its own system are not complicated; each part has within itself, the several structures necessary for its own existence, so that a distinct organ like the medulla spinalis is dispensed with, and only so much of a structure resembling the nervous system of animals is required, as is adequate to furnish to each part a power of motion; and this may be so interwoven with its other structures, and so much resemble them, as completely to elude our research.*

M. Le Gallois, says the Reviewers, has shewn with other good physiologists, that the power of the heart is not derived from the brain. M. Le Gallois has done more; he quitted the field of conjecture and resorted to that of experiment; and guided by facts alone, has shewn whence that power is derived. He shewed, that it was not from the brain, because it continued after the removal of that organ; but concluded very naturally, that it was from the spinal marrow, because it does not survive the destruction of that organ. We now have explained, what has hitherto caused much perplexity, why the heart is so largely supplied with nerves.* It receives branches from the par vagum, branches directly from the medulla spinalis, and from the sympathetic nerve; so that it is connected with the spinal marrow as a whole. It is not dependant on any particular portion for its supply of power, but by means of the sympathetic, it can derive it from every and any portion. But from this arrangement the reviewers raise an objection to the doctrine. The destruction of no particular part of the medulla spinalis will cause to cease the power of the heart; but with respect to the muscles, the destruction of that portion, from which they derive their nerves, causes an immediate abolition of their power.† Hence they conclude, that the influence, which the medulla spinalis exercises over the heart, is different in kind from that, which it exercises over the muscles. But the difficulty is only apparent. The muscles lose their power, because they are dependant for it, immediately on that portion, from which their nerves proceed. The heart, on the contrary, has through the sympathetic a resource in the whole spinal marrow, and so long as any portion remains unmolested, it may receive from that portion a modicum of power. The influence, therefore, exercised in these cases, is not different in kind, but only in degree; it is extended to the heart from every point of the medulla, but to other muscles only from a single point.

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* We do not suppose the ultimate distribution of the nerves in large animals to be bounded by the ability of our organs to pursue it. In animals without vertebræ, we begin to lose sight of a nervous system, and in those of a lower grade it completely escapes us; but the fair conclusion from analogy appears to be, that even these animals possess it.

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* The nuemrous distribution of nerves to the heart, will not be discovered without minute and careful dissection. According to the representations of Mr. Scarpa, who is allowed to be a most faithful anatomist, they form almost a continued plexus around that organ.

The intenseness of the power of the heart over that of any other muscle is generally noticed But can this fact be explained, admitting this power to be an attribute of the muscular fibre? Why should one muscular fibre possess more of this property than another? With respect to their inherent properties, they must be subject to one law. On the contrary, if this power be the effect of the nervous influence, the solution of the phenomenon is at hand. The intenseness of the power of the heart is the consequence of the more bountiful supply of nerves to this, than to any other muscle.

† The words immediate and instantaneous, I have used for the sake of conciseness, but they should not be used without some qualification. When the muscle is closely in the vicinity of the medulla spinalis, and its nerves of course are short the abolition of the power of the muscle immediately follows the destruction of the medulla; but if the nerves are long, or proceed to some distance from their origin, as in the muscles of the limbs, some minutes will elapse before all signs of motion disappear. Is not this owing to the nerves being reservoirs of power? From this circumstance we derive an explanation, why the heart exhibits some motion, after it has lost its ability to carry on the circulation, and even after it is torn from the body and laid on the table; it is, I apprehend, in consequence of a store of nervous power, which it has on hand, and the motion

It does not amount to a demonstration, that, because the heart has nerves, it is dependent on their influence for its power; but this circumstance connected with the effect of the destruction of the medulla spinalis is of much consequence. These nerves have always been a stumbling block in the way of the advocates for the independent power of the heart. No very plausable use could be assigned to them; and it has been a cause of much dispute and various hypotheses. The ingenious writer of the Review, by his sympathy has brought to their aid a happy supposition; and has contrived an office for these nerves, which would be tolerably satisfactory, had not their real intention been ascertained. These nerves, he believes, are supplied to the heart, "as organs of communication, or of sympathy, with the other various parts of the body;" and the explanation of the effect on the heart of the destruction of the medulla spinalis is to be found in the interruption of this sympathetic relation. The exsistence of such a relation between different organs, and between the heart and some of these organs, cannot be doubted. The principle by which this relation is maintained, is not generally recognised with sufficient distinctness; it is undoubtedly the case of many phenomena, and of the first importance in the animal œconomy; but it does not seem to be admissable for the explanation of the phenomenon, to which

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continues, till this is exhausted. M. Le Gallois, thinks that these insufficient motions are the effect of its irritability, still unwilling to resign the once favoured notion. But there seems no necessity, for multiplying cases when one is sufficient.

It has sometimes happened in my experiments, that the heart would be able to effect the circulation for three or four minutes after I presumed I had destroyed the medulla spinalis, though in most instances the power necessary for that purpose was instantly arrested. May not these exceptions have been the consequence of an accumulation of nervous power in the sympathetic nerve and its ganglia?

this writer has assigned it. The death of the heart as a consequence of the destruction of the medulla spinalis is admitted; but it dies from sympathy. With what organ does it thus sympathize? Nothing but the medium of communication is injured. Why does it not die, when a portion, two thirds or three quarters for instance of this medium is destroyed; are not the destruction of the relationships sufficiently numerous? It does not die till the whole spinal marrow is annihilated. But it is probable, that even the breaking up this chain of sympathetic relation with other organs, should have an effect of such magnitude, as to prostrate even the *inherent* power of the heart? Is the heart so dependent on this relation with other organs for the performance of its function? This has not been shewn; in fact, the contrary is proved. It appears from the experiments of M. Le Gallois, that all the viscera except the lungs, that the head and all the trunk, except so much of the thorax, as is sufficient to contain the heart and continue its connexion with a portion of the medulla spinalis, may be removed, and yet by performing artificial respiration, the action of the heart may be maintained for some time. Aware of these difficulties, the writer supposes a sympathy to exist between the heart and the medulla spinalis. But this is not proved; and it does not seem necessary for the purposes of the animal œconomy. It is merely supposition; a foundation for theorising, which ought to be admitted with great hesitation. But admitting the reality of this sympathy, what proof have we, that the destruction of one organ should cause the absolute destruction of another, with which it sympathises. The most that ought to be expected to take place, would be a suspension of its action, not an annihilation of its power. For such an effect as is contemplated by the reviewers, have we any evidence from analogy? Does it happen between organs possessing the most intimate sympathy? It is true, that "severe affections of the mind, and blows on the pit of the stomach," sometimes suddenly produce death; but in these cases, I imagine rather that a suspension of action takes place in the vital organs, than an annihilation of their powers. A blow on the pit of the stomach arrests the action of the heart probably, by first suspending respiration, and not by annihilating its power of motion. A delicate sympathy exists between the heart and the brain; but a sudden destruction of the one, does not paralize the other;

its function only is suspended, and it can be re-excited by the application of its natural stimulus.

By destroying the spinal marrow, exactly the same state is produced in the voluntary muscles, as in the heart; they not only cease to be obedient to volition, but their power of motion is obliterated. Is this the effect of sympathy or the loss of the source of this power?

Much stress is laid upon the suddenness with which the destruction of the medulla is accomplished, but the same ultimate effect takes place with respect to the heart, whether it be done suddenly or slowly. But this ought not to be the case, were sympathy the cause. One of two organs which sympathize, may be removed without materially injuring the other, if it be done carefully. The heart is affected in proportion to the loss of the source of its power. If by means of an instrument introduced into the medulla, the destruction be effected gradually, by small portions at a time, the injury will not extend beyond the contact of the instrument; but if it be done by a sudden and violent blow, the injury will extend much beyond the reach of the instrument; and sometimes it may happen, that a complete disorganization of the medulla will be produced, and in consequence, the death of the heart ensue. The heart feels more sensibly the loss of the cervical, than that of the dorsal portion of the medulla; does it sympathize with the first more keenly than with the last?

It is stated truly, that if the circulation be stopped in a part of the medulla, by tying the vessels which support it, death ensues in that part very soon; and if this dead part be torn by an iron probe, the heart is not affected by the violence; for says this writer, "the heart will not sympathize with a dead part." The fact admits of a different explanation. The heart had already suffered to the full extent, which that loss could cause it to suffer; the life of the part having been extinguished, it could be of no farther use; no additional violence could more effectually kill it, nor add any thing to the affection of the heart. But let us pursue the other explanation. If we kill one third of the medulla by stopping its circulation, we may lacerate it as much as we please, and not affect the heart, because it will not sympathize with a dead part; we may then kill another third, and the same effect will follow; we proceed to kill the remaining third in the same gentle way, and we may then commit any violence on the whole medulla spinalis, and the heart will not be effected, "for it will not sympathize with a dead part." This is what ought to happen, if no other relation than the sympathetic, existed between the heart and the medulla; the heart ought to continue its action after the removal of the medulla with a power little diminished. But this does not happen; and I therefore conclude that the death of the heart by the destruction of the medulla spinalis, cannot be accounted for upon the principle of sympathy, and we must resort to some other explanation of that phenomenon.

From the statements which we have made, it seems that the influence of the medulla spinalis does militate with the doctrine of irritability, and in fact completely subverts it. The basis of this doctrine is the phenomena, which the heart presents after the apparent death of the animal. The heart even after decapitation, and the suspension of respiration. continues to act with so much force, as to carry on the circulation of the blood for some minutes; and if artificial respiration be performed, this action may be continued for some hours. The brain was supposed by Haller and his followers, to be the only source of nervous influence; to prove therefore that the function of the heart continued without the brain, was to prove it independent of that influence, and as no other source was suspected, from which the heart could receive its power of motion, and its property of being excited by stimuli, it was concluded that these were inherent properties of the muscular fibre; and what confirmed this opinion, was the circumstance of the heart continuing its motions for some minutes after its entire separation from the body. But with respect to these motions, a material point was overlooked; it was not observed that they were not movements of force, and that they were inadequate to the accomplishment of the function of the heart. But the experiments of M. Le Gallois prove, that the brain is not the only cause of the nervous power; but that the medulla spinalis partakes with it in that function; that the reason why the heart continues to act after decollation is, because it is still connected with the source of its power, the medulla spinalis; and that the destruction of this, reduces the heart to the same state, as when it is separated from the body. It loses its ability to affect the circulation of the blood, and it is only capable of a few feeble efforts, which soon expire.

This should not be the case, did there reside in the heart itself a power of motion, "independent of any agency or influence of the nervous system;" but on the contrary, the heart should be as able to continue its function after the loss of the medulla spinalis, as after the removal of the brain. At least it is inherent on those who contend for the independent power of the heart to explain the effect of the destruction of the medulla spinalis consistently with the existence of such a power.

The author of the Review has assigned to sympathy the explanation of another phenomenon, which though it has no connection with the question which has engaged us, is worthy of some attention. It is well known, that if the medulla spinalis be divided above the origin of the phrenic nerves, the muscles of respiration immediately cease to act. The commonly received opinion as to the cause of this fact, has been, that these muscles being voluntary, and the communication between them and the will being interrupted by the division of the spinal marrow, they are paralized in the same way as are other voluntary muscles. Some physiologists have supposed that they are not exclusively voluntary, but are of a mixed character, since their function is for the most part performed without the consciousness of the animal. The author of the Review has adopted the latter opinion, and ingeniously explains the action of these muscles upon the principle of a sympathetic relation existing between them and the lungs. "The muscles of respiration," says he, "are excited to action, not by a stimulus applied to themselves, but by a stimulus existing in the lungs; by a necessity in those organs for a change of air." An intimation of this necessity is communicated along the par vagum to the medulla oblongata, whence by sympathy these muscles are excited to action through the medulla spinalis. This chain forming the only medium of communication between these muscles and the lungs, the division of the medulla spinalis destroys the sympathetic relation, and the muscles cease to act. So far it seems very easy; but the par vagum is as important a link in the chain as the medulla, and we as effectually interrupt the communication of sympathy by the division of the first, as the last. The muscles can no longer be influenced by any suffering of the lungs; but they do not cease their efforts; on the contrary, these are redoubled with a view to relieve the distress, caused by the failure of the lungs to perform their part of the function. These efforts are continued for some hours, and in fact, till death takes place, in consequence of the failure of the lungs, or by some other means. Hence it appears that if sympathy has any agency in producing the motions of the respiratory muscles, it is not the chief cause.

The cessation of the movements of the muscles of the respiration in consequence of the division of the medulla spinalis presents, says the Review, a great difficulty, if Le Gallois's doctrine, that the muscles derive their power from the nervous system, be true. And Le Gallois himself says, "it is one of the greatest mysteries of the nervous power."* If it be true, that these muscles are of a mixed character, partly voluntary and partly involuntary, the difficulty would be as yet inexplicable; but if they be altogether involuntary, it would be no greater with them than with other voluntary muscles. The will ceases to command them, and they cease to act. But they do not lose their power; and if the communication could be restored between them and the will, they would undoubtedly again do their duty. It is not proved that these muscles are in any degree involuntary. The principal circumstance in favor of the supposition seems to be that we are not conscious at all times of the act of volition, which directs their movements. This does not, however, appear to be conclusive when we consider of how many volunary acts we are perfectly unconscious, or at least of which the consciousness is so faint as to make no impression. How many changes of posture do we make, when asleep, not to mention somnambulism, which does not in the least recognise?

We learn from the experiments of M. Le Gallois, that the medulla oblongata, or rather a part of it, is the organ appropriated to the function of respiration; and the will through this directs the actions of several voluntary muscles, necessary to the accomplishment of that function. In cases of great necessity it calls in the aid of muscles not commonly employed in that process, particularly those muscles, which move the jaw, the mouth is stretched open, that a greater volume of air may be taken in at a time. After the division of the medulla spinalis, and the consequent sessation of the motions of the proper muscles of respiration, to supply their loss, this effect is made by alternately opening and shutting the mouth, like a frequent gaping. This phenomenon, as well as the mechanical phenomena of respiration, is said to be the effect of sympathy. The sympathetic influence, it is said, is communicated from the lungs by the par vagum to the medulla oblongata, through which it causes the motions of these muscles by the nerves, with which they are supplied. But these muscles are altogether voluntary ; nothing of a mixed character has ever been assigned to them. Can the voluntary muscles be subjected to the agency of any other principle, than the will, and not be taken from the contract of the will ? When not under the control of the will their actions are irregular or spasmodic. But the actions referred to are not spasmodic ; they are as regular as in an uninjured animal. If sympathy does any thing in this case, it must be by the will ; the will may be influenced by the general distress, which by exciting these motions it endeavours to relieve. Besides, these motions continue after decollation, when the medulla oblongata can no longer be influenced by the lungs. The cause of the sympathetic action being removed, we should no longer expect the effect. But it may be said, that the motion being already impressed on the medulla, would continue after the removal of the cause ; were this admitted, it could not be supposed to continue for ten or fifteen minutes, which is the duration of the gaping. Some other agent therefore than sympathy, must produce these motions ; what other is there, but the will ? It is not improbable, that the will may influence parts, which have been subject to its control, while life remains in them. But is there volition without conciousness ; or is there perception of sensation in this state of things ? There probably is an indistinct consciousness of uneasiness in the head, arising from the loss of the circulation of arterial blood ; or the head may refer this sense of uneasiness to the separated trunk, as is the case after amputation, the sense of pain produced by an offending cause at the end of the stump is referred to the extremity of the separated limb. Be this as it may, it does not appear, that any other agent, than the will can produce the regular motions, which these voluntary muscles perform.

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* Page 50, English translation.

The reviewers commend their author for some good done to physiology ; it is to be lamented, that they do not coincide with him in his other, and most important conclusions. They would then join in the sentiment, that the discoveries of M. Le Gallois, form a new *epoch* in the science of physiology. A new field for inquiry is opened to us. For supposition, is presented positive knowledge. The culture of a most important part of the science is wrested from the control of fancy and subjected to the senses and the judgment. The nervous system is raised from obscurity and exhibited in its real importance. That part of it called the medulla spinalis, which we have imagined to be merely a bundle of nervous cords or wires, or any thing else, that fancy suggested, without being able to assign to it any certain use, is proved to perform an important function in the animal œconomy. Nature, true to herself in this, as in every other case, did not furnish a complicated apparatus to accomplish a single object. The attainment of her ends are always commensurate with the extensiveness of her means. The light thus given to physiology cannot fail to extend its influence to pathology, and we may anticipate, by means of the facts promulgated by M. Le Gallois, considerable improvement in the art of medicine.

December 9*th*, 1814. W.

INTELLIGENCE.

Two Cases, with Observations, demonstrative of the Powers of Nature to reunite parts which have been, by accident, totally separated from the Animal System.

BY WILLIAM BALFOUR, M. D. OF EDINBURGH.

[From the Edinburgh Medical and Surgical Journal for October, 1814.]

THE practice of ingrafting trees first suggested to medical practitioners the idea of repairing mutilated parts. This practice was very successfully prosecuted by a few, when the state of society afforded opportunities, but has uniformly been treated with a certain degree of ridicule, by far the greater number of the profession. This distrust in the powers of nature, in the face, too, of evidence which would be difficult to controvert, is not more unphilosophical in itself, than its influence on practice must be detrimental. What a man believes impossible, he never will attempt. But if a branch of one tree unites with the stump of another, so as not only to live, but to bear fruit, it can be no great stretch of imagination to suppose, that a part totally separated from the animal system, may under proper management, reunite and live, and perform all its proper functions.

A number of years ago, an accident happened, in the management of which I proceeded upon the principle, of the possibility of parts entirely separated uniting again, with all the success I could desire. This idea was suggested by the necessity of doing something without delay, and the recollection of the Taliacotian mode of artificial noses. At that time, however, I knew of no case in which even an attempt had been made to replace parts, as near being separated from the system as were those in this instance. This case I did not publish at the time it occurred, for many reasons hereafter to be mentioned. Indeed it had almost gone out of mind till a fresh accident occurred, the cure of which by reunion ranks, under all the circumstances of the case, among the most wonderful instances of the powers of nature, and for ever sets at rest the question, "Whether parts, which have been completely separared from the rest of the animal system, and in which circulation had ceased altogether, can be again reunited?" There is a circumstance, too, which stamps a value and importance on the two following cases, above all, or most others of the kind; that is, their authenticity, or the proof that can be led, that the facts recorded really happened precisely as recorded; a proof, which, from the number and respectability of the witnesses, must convince the most sceptical.

The recollection of what took place in the first case, left little doubt in my mind of an equally favourable result in the second; for I never attributed the least degree of the success attending the former, to the small slip of skin that remained undivided. It appeared to me highly improbable, that a connection so very slender could keep up circulation betwixt the system and the separated parts, with a vigour sufficient to keep the latter alive.

From the moment, therefore, I found reunion to have taken place, I began to suspect, that the precautions employed by Taliacotius, and his contemporaries, to keep up circulation in parts destined to supply the place of others, were necessary, and inadequate to the purpose. Can it be supposed, for instance, that an extensive surface cut out of the arm for forming an artificial nose, could be fed by a small attachment, with a vigour at all proportioned to the celerity with which adhesion by the first intention takes place? It is impossible. Suppose a piece of skin, of such extent, were raised from any part of the body, and allowed to remain, without being connected with any other raw surface, how long, I ask, would that part of its margin, most distant from the attachment left to keep up circulation in the part, live? I am convinced very little, if any blood, would issue from it except what might be in the vessels at the time of their being divided.

If, therefore, the connection left to keep up the circulation of the excised parts, in Taliacotius's operations, was insufficient for that purpose, these parts must be considered as having been in the same situation as if entirely separated from the system, and their adhesion to the parts to which they were applied as having depended, not on the circulation supposed to have been kept up in them, but on the principle of vitality remaining in them, and on the circulation having been restored to them, by the new surface to which they were attached.

With regard to circulation being kept up, in parts cut out of the forehead, as now practised in India,* it is out of the question. The twist given to the small attachment, left at the root of the nose, must almost entirely preclude any thing of the kind.

I am convinced, therefore, that had Taliacotius at once separated from the system, the flaps of skin with which he repaired mutilated parts, his operations would have been equally successful, infinitely less troublesome to himself, and distressing to his patients. With this conviction, arising solely from the success attending the first of the following cases, I was resolved to attempt the reunion of any divided parts that might come in my way, unless such parts were of a magnitude that the opposition of the wounded surfaces would not restrain hæmorrhagy.

There are instances on record, with not one of which was I acquainted when the accident happened to my son, of the *points* of fingers, ears, noses, being nearly or entirely separated from the system, that were made to readhere. But Pedie's case, so far as I know, is without a parallel; and I have the authority of a number of the first characters in the profession in this place to say, that it is the most extraordinary that has come to their knowledge. This is my apology, if any is necessary, for giving it to the public. For "it must not be imagined, that the recital of such uncommon cases is without its use; for, while they extend our knowledge of the powers inherent in living bodies, they inform us of the advantage often to be derived from allowing these powers proper opportunities of exerting themselves. Of reunion, by adhesion, we are, in no case, to despair, so long as the least degree of circulation remains in both, or even in one of the parts divided."* To this I would add, that many things are left undone, from mere supineness, or a belief that they are impracticable, because they were never known to have been done before. Of what advantage then may it be, to be generally known, that such things as are detailed in the two following cases, are not only practicable, *but have been done?*

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* A thin plate of wax is fitted to the stump of the nose, so as to make a nose of a good appearance. It is then flattened, and laid over the forehead. A line is drawn round the wax, and the operator then dissects off as much skin as it covered, leaving undivided a small slip between the eyes. This slip preserves the circulation till an union has taken place between the new and old parts. The cicatrix of the stump of the nose is next pared off; and immediately behind this raw part, an incision is made through the skin, which passes around both *alae*, and goes along the upper lip. The skin is now brought down from the forehead, and, being twisted half round, its edge is inserted into this incision, so that a nose is formed with a double hold above, and, with its *alae* and *septum* below, fixed in the incision. The connecting slips of skin are divided about the twenty-fifth day, when a little more dissection is necessary to improve the appearance of the new nose.—*Gentleman's Magazine for October*, 1794.

Individuals in every line of life, but especially those who work with edged instruments, or who are employed about machinery, would often have it in their power to prevent unsightly mutilations in themselves or others, and be enabled to earn their bread as before. Surgical skill is not necessary in all instances of such accidents. The chief thing wanted is, a conviction that attempts at reunion of divided parts *may* be successful: for whoever has this belief will not fail, when such accidents occur, to give opportunity to the powers of nature to exert themselves. There is, indeed, no saying what may be done in this way. It would be unphilosophical to set bounds to the powers of nature. And because noses, ears, and the *points* of toes and fingers, are the only parts which, when separated, are known to have reunited, is nature to be blamed for that? The reason is, the reunion of other parts has never been attempted. I would not, however from these observations, be understood as imagining, that parts of very considerable magnitude, when totally separated from the system, can ever be expected to reunite. The impossibility of this, I think, evident, *à priori.*

But the knowledge of such facts as occurred in the following cases, must, by increasing his confidence in the powers of nature, induce every rational practioner to trust more to them, in many circumstances, than he otherwise would do. Therefore, though it would be madness to expect the reunion of a leg or an arm that had been entirely separated, yet I can very easily conceive a leg or an arm to be wounded in such a way, by accident, or in battle, as, according to received principles, to render amputation necessary, but which in the hands of a surgeon, quite aware what nature can do, might be preserved. I can very easily conceive that, to a practitioner who knows that nature, by her own innate powers, unaided by a single auxiliary artery, can effect the reunion of a finger that has been entirely separated from the system for nearly half an hour, cases may occur, in which he will pause before he takes off a limb, which, but for such knowledge, he would amputate without hesitation. In this view, Pedie's case in particular, will, I trust, be an acceptable present to surgeons in the army and navy where casualties occur so often, and of course there are such opportunities for observation, and warrantable experiment, and may be the cause of preserving limbs which otherwise would be lost, to many a gallant man. At all events, it is evident, that the practice of attempting the reunion of separated parts, may be carried farther than has ever yet been done; and it must be a comfortable reflection to an unfortunate sufferer, in the hour of accident, to know, that a whole finger, or even all the fingers of a hand, though entirely cut off, may be restored.*

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* Dr. Thomson's Lectures on Inflammation, page 243.

CASE I.

About eleven years ago, Mr. Gordon, surgeon, now, I believe, in India, after having conversed with me for some time one day, in my shop, upon going out shut the door smartly after him, without perceiving any body near it. Unfortunately, one of my sons, a boy of about four years and a half, diverting himself on the outside, had one of his hands in the groove of the hinge side of the door. I was shocked with a wild scream that I heard upon the door being shut; and still more so, when Mr. Gordon came in, carrying the boy in his hands, stretched, from agony, as upon a rack. The points of three of his fingers were completely separated, with the exception of a slight attachment of skin, which barely suspended the parts. The points hung at right angles when the fingers were extended. The point of the index was cut off at the middle of the nail, the fore-finger a little above the nail, and the ring-finger at the root of the nail. The wounded surfaces were necessarily much bruised, but the fingers were nevertheless, cut so perpendicularly, that, unless I had seen it, I could not have believed a door could have done it. With the assistance of Mr. Gordon, the innocent cause of the accident, I instantly replaced the parts, with but little hopes, I confess, owing to the degree of contusion of the wounded surfaces, of reunion taking place. But I was so shocked at the idea of the boy's hand being mutilated for life, that I hesitated not a moment to put the powers of nature to the test. On the sixth day after the accident I removed the bandages, when I found adhesion had taken place, to the unspeakable joy of Mr. Gordon, the boy, and myself. The skin and nails came off all the three fingers, but were afterwards renewed; and the cure was so complete, that a narrow inspection was necessary to discover any difference between the fingers of the one hand and those of the other. There was, indeed, no difference to be perceived, but a slight scar on the left side of the ring-finger, at the root of the nail. This case I certainly would have published at the time it occurred, but on Mr. Gordon's account, who, though not the smallest blame was attributable to him, suffered more anxiety and distress of mind than I did myself, and never liked to hear the subject mentioned. I trust he will now excuse me for mentioning him by name, having no other motive for so doing, than the establishment, beyond the possibility of contradiction, of the truth and accuracy of the above statement. Mr. John Moffat, accountant of Excise, Mr. Alexander Milne, surgeon, now on board the Norge, 74, and my servants, were likewise witnesses of the facts. The boy died of the scarlet-fever, a year and a half after the accident; and but for the following case occurring, which to most, I am sensible, will appear much more interesting and decisive, that of my son would never have been recorded.

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* A gentleman, to whom I told Pedie's case lately, expressed his regret that he had not known it sooner; "for," said he, "our blacksmith, a very clever fellow, a short time ago, struck off three of his fingers, from about the middle. He ran immediately, with the pieces hanging by small slips of skin, to the surgeon of the village, who out with his scissars, divided the slips of skin, threw the fingers away, and contented himself with dressing the stumps." Now, this gentleman did what almost every other surgeon would have done; but had he known Pedie's case, the blacksmith, a thousand to one, would have had his fingers to-day.

CASE II.

On the 10th day of June last, two men came into my shop, about eleven o'clock, forenoon, one of whom, George Pedie, a house-carpenter, had a handkerchief wrapped round his left hand, from which blood was dropping slowly. Upon uncovering the hand, I found one half of the index wanting. I asked him what had become of the amputated part. He told me he had never looked after it, but believed it would be found where the accident happened. I immediately dispatched Thomas Robertson, the man that accompanied the patient, to search for and bring the piece. During his absence I examined the wound, and found that it began near the upper end of the second phalanx, on the thumb side, and terminated about the third phalanx on the opposite side. The amputated piece, as measured by the patient himself, was an inch and a half long, on the thumb side, and an inch on the other. The wound was inflicted in the cleanest manner, by one stroke of a hatchet, and terminated in an acute point.

In about five minutes, as nearly as I can guess, Thomas Robertson returned with the piece of finger, which was white and cold; and I remarked to Dr. Reid, who was present, that it looked and felt like a bit of candle. Without the loss of a moment, I poured a stream of cold water on both wounded surfaces, to wash away the blood from the one, and any dirt that might be adhering from the other. I then applied, with as much accuracy as possible, the wounded surfaces to each other, expressing a confident expectation that reunion would take place.

I endeavoured to inspire the patient with the same hopes, by detailing to him the success I had in my son's case, which, for the reasons already mentioned, was to me quite decisive of the question, whether or not parts entirely separated from the system would reunite? All this was heard by the patient with a very apparent distrust. But I could do no more than tell him, that, if reunion did not take place, no harm could ensue from the attempt, and that, if it did, a great deformity would be prevented. I informed him, that unless pain or fetor, or both, should occur, I would not remove the bandages for a week at least; directed him to keep his fore arm slung, and not to think of any kind of work. At last he entered so far into my views as to promise punctual obedience. He called on me next day, when he felt no particular uneasiness, but remarked, that the wound had not altogether given over bleeding. Assuring him there was nothing in that, desired him to call on me every day; but did not see him again till the 4th of July! Concluding, from his absenting himself without assigning any reason, that he was one of those, too frequently to be met with in the lower ranks, who go from one medical man to another, just as their fancy strikes them, or as they happen to be advised by some of their foolish and ignorant neighbors, and whose ingratitude to any practitioner is in exact proportion to the good he does them, I suspected he had fallen into bad hands, and that I never would hear more of him. On the 2d of July, however, a gentleman called on me, and asked if I recollected a man who had got a finger struck off, about three weeks before, to have come through my hands?

I told him I recollected perfectly well; that I was filled with indignation at the fellow's unreasonable and ungrateful conduct; and that I was just about setting on foot a search after him, not having informed myself either of his name or place where he was employed, at the time he applied to me. The gentleman said he would save me the trouble, for he could give me an account of the man.

The accident happened on the 10th of June, and on the 12th, the patient, under the influence of the ridicule of his acquaintances, for giving the least credit to my assurances that reunion would take place, applied to another practitioner. This gentleman, I am informed, on being told the object I had in view in replacing the piece of finger, represented the impropriety of any other person intermeddling with it. But, prepossessed with the belief that he carried about a piece of dead matter only, tied to the stump of his finger, the man insisted on having the bandages removed, which was done accordingly. Thus were nearly rendered abortive, my attempts at the reunion of the parts, and the profession deprived of a fact, which, as demonstrating the wonderful powers of nature to repair injuries, is inferior in importance, to none in the annals of the Healing Art. But, fortunately, nature had been too busy for even this early interference to defeat her purpose. ADHESION HAD TAKEN PLACE.

In consequence of the information I got from the gentleman who called on me the 2d of July, I found out the patient on the 4th, when reunion of the parts was complete. The finger, in fact, is the handsomest the man has, and has recovered both heat and sensation. In the progress of the cure, the skin was changed, and soon after the accident, the nail fell off; but I have not the smallest doubt that it will likewise be renewed.

From the information obtained, not only from the patient himself, but from those present when the accident happened, I am satisfied, that upwards of twenty minutes must have elapsed before the parts were replaced. For the patient did not apply to me *immediately* upon receiving the injury. He waited on the spot till a great number of his fellow workmen, separated in different apartments of a large building, came to see and condole with him on the occasion. The word *immediately*, in his affidavit, must therefore be understood as so qualified.

I have thought it proper to subjoin the affidavits of George Pedie, Thomas Robertson, and Dr. Reid, to the principal facts and circumstances of Pedie's case, that no doubt might remain of their truth and accuracy. For "it must be confessed, that instances of reunion among parts which had been entirely separated, are very rare in the human body; so rare, indeed, that most practioners still treat with disbelief and ridicule, the few instances which have been put upon record."* These affidavits are still more necessary to convince people who are not of the medical profession, but to whom the knowledge and belief of such facts may be useful. Numbers of such having heard an imperfect account of Pedie's case, have called upon me to ascertain the truth; but I have never yet met with one who expected me to confirm the facts, of the *entire* separation and *complete reunion* of the parts.

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I, George Pedie, house carpenter, declare, That, on the 10th day of June last, when at work in the Advocate's Library, I accidentally struck off the finger next the thumb of my left hand, at one stroke, with a hatchet: That, accompanied by Thomas Robertson, foreman of the work, I immediately went to Dr. Balfour, who, as soon as he saw what had happened, asked where the bit of finger was that had been struck off? That, I said, I did not know, but believed it would be found where the accident happened: That, Dr. Balfour requested Thomas Robertson to go and bring it as quickly as possible: That, Thomas Robertson went and returned with it in about five minutes: That, Dr. Balfour immediately washed both it and my bleeding finger with cold water, and replaced the piece that had been struck off, and bound it up: That, Dr. Balfour said he expected it would adhere, because he had been successful in a similar case eleven years ago, having replaced three of one of his son's fingers that had been cut off by accident, and which completely united: That, I had no reason to go to any other than Dr. Balfour, but that I did not believe the part of my finger that had been cut off would reunite, and that I was laughed at by all my acquaintances for ever expecting that it would: That, when the dressings were first removed, which was on the 12th of June, reunion of the parts was found to have taken place. And I declare, that the merit of the cure belongs exclusively to Dr. Balfour. All which I declare to be truth.

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* Dr. Thomson's Lectures on Inflammation, page 239.

(Signed) GEORGE PEDIE.
DUNCAN COWAN, J. P.

Edinburgh, 18*th July*, 1814.

Edinburgh, 19*th July*, 1814. Compeared Thomas Robertson, mentioned in the preceeding declaration, who being examined, declares conform to the preceding witness *in omnibus*. And this is truth.

(Signed) THOMAS ROBERTSON,
DUNCAN COWEN, J. P.

I, PETER REID, physician in Edinburgh, declare, that I was witness to the facts and conversation stated in the above declaration by George Pedie; that I have this day examined his finger, and find that complete reunion has taken place.

(Signed) PETER REID, M. D.
DUNCAN COWEN, J. P.

Sworn before me, at Edinburgh, July 26, 1814.

From the above details, many questions naturally arise, any one of which I am far from pretending satisfactorily to answer. It is impossible, however, to dismiss such a subject without hazarding some observations.

What, then, is the process which nature follows in re-establishing a connection betwixt the animal system, and a part that has been entirely separated from it? It is agreed upon, as the result of observation, however inexplicable the facts may be, that when two recently divided surfaces, both of which are still connected with the system, are applied to each other, with a view to immediate adhesion, or reunion by the first intention, a layer of gluten is first interposed between them. It is reasonable to suppose, that both surfaces contribute equally to the formation of this layer, which, soon after, is seen to be penetrated with blood-vessels. These vessels, however, are not unconnected, in any stage of their existence, with the surfaces. They do not begin in the substance of the layer and advance of the surfaces. They begin at the surfaces and advance towards each other; or, more properly speaking, they are the blood-vessels which had been divided, now elongated, through the medium of the organizable fibrin, for the purpose of reuniting the parts. Not so with parts that have been entirely separated from the system, and in which circulation has ceased altogether. The fibrin, in this case, must be effused from one surface only, that connected with the system. The vessels of this only can be elongated; and those of the separated part must be nearly passive in the process of re-establishing circulation. The separated part must be considered in a state of suspended animation, still possessing excitability. When new blood comes in contact with the open mouths of its vessels, it is probable that it is absorbed by capillary attraction. This new blood, being the proper and natural stimulus to its own vessels, must excite the dormant vessels to action; and upon this action must depend the connection that is formed between them and the vessels projected from the living surface. Circulation between the surfaces being thus established, must be gradually extended through the whole part that had been separated, by the *vis a tergo*, and the action of its own vessels.

Analogous to this is what happens to persons who are recovered from drowning. Though in them all the functions are suspended, the body is not dead. The principle of vitality still remains. It is, indeed, difficult absolutely to say when this principle is extinguished; at least, nothing short of the formation of new combinations can warrant the conclusions. Thus, though the body may have been a considerable time immersed in water, and is to all appearance dead, if the circulation, of the blood can be restored, by the gradual application of heat, friction, and artificial respiration, it becomes reanimated, and is restored to the exercise of all its functions.

Report on the Use of Charcoal Powder as a substitute for Cinchona.

BY R. CALVERT, PHYSICIAN TO THE FORCES.

[From the Edinburgh Medical and Surgical Journal, for October, 1814.]

SIR—When I sent you a former paper on the subject of charcoal powder, which I see published in your number for January, I intended to have prosecuted the inquiry, and to have sent you the result of my experience from time to time, in order to confirm or disprove, as such experience might dictate to me, what I had therein stated. But being called to Malta immediately afterwards, in consequence of the plague that infested that island, I have had but little opportunity of putting the virtues of charcoal to the test in intermittent fevers. The investigation, however, has in no wise been retarded in Sicily, from my leaving it. Dr. Calcagno, to whom I am indebted for the discovery, has published the result of his further experience on the subject, in a letter "Sull' use interno del Carbon di Legno," &c. wherein he relates his having cured a double tertian in the person of Giuseppe Bellavia, a patient in his hospital, by administering the powder of charcoal in the dose of a drachm every hour during the intermissions, but without the addition of an acid, which he formerly thought necessary. The fever did not return after two ounces of the powder had been administered.

From the 23d September to the 13th October, he had an opportunity of employing it in five other cases, in four of which the remedy succeeded by the time two ounces and a half had been given; but in the fifth, the fever not being removed after the exhibition of three ounces, he judged it prudent to have recourse to bark, which prevented the following paroxysm, although but half an ounce had been prescribed. The period of this paroxysm being over, he again ordered the charcoal, and the patient continued well.

Maccadino, his pupil, on returning to Calafimi, his native place, in the month of August, succeeded in curing eight cases of intermittent fever with the same remedy; and Signior Francesco Buscarelli, surgeon, following his example, found it useful in four other cases of the like nature.

"In the district of Aliminusa," says Dr. Calcagno, I know that the Journal* had scarcely arrived, when the practice became general, the inhabitants adopting it (the charcoal) as well without the prescription of the physician as with it; and these write to me in a letter, bearing date the 23d December, "That the patients labouring under intermittent fevers that have been cured at this place by means of the powder of charcoal amount to a hundred and five; that each has not taken more than two ounces of the charcoal; and that the charcoal made use of was that prepared from the *stinco*." Dr. Nicosia also employed it in seventeen other cases in the district of Nissoria, according to the testimony of Dr. Burza of Palermo.†

But, however assiduous the Sicilians may have been in taking advantage of a remedy they very much required, they have not been without rivals in many British practitioners, under the auspices and control of Dr. Borland, inspector of army hospitals. Mr. Mackesy, surgeon of the 62d regiment, has permitted to be published in Italian, seven interesesting cases, in which the charcoal was successfully employed by him; but, as these cases will probably be published in England, I shall not enter into particulars respecting them. Mr. Tully, surgeon of the 35th regiment, in a report made to the inspector of hospitals, dated Zante, Nov. 12, 1813, states, that he has found the remedy particularly successful in remittent and intermittent fevers of that island; and that its administration, in more than thirty cases, principally intermittents, has been attended with the most happy effects. "In obstinate cases of diarrhœa, and in the last stages of dysentery," he adds, "I have seen that the charcoal, in doses of from 15 to 20 grains given three or four times a day, is a most excellent remedy." This accords with my own experience, as related in the Journal of January. I shall relate a complicated case of intermittent fever and dysentery that occurred to me. It is to be remarked, however, that I did not prescribe for the man till the 29th July.

Case.—Adam Hopffe, 20th Light Dragoons, aged 37, orderly servant in the hospital, began to feel weak and languid in the middle of July, but did not complain till a week had elapsed, when, after attending a patient who died of dysentery, he was seized with headach, irregular chills, and other febrile symptoms, accompanied with a frequent purging of blood. He then took, by order of the assistant, a dose of salts, which operated well. The purging continuing the next day, he took a grain of opium in the morning, and another in the evening. This gave him but temporary relief. Two days afterwards he took 15 grains of rhubarb, but the dysenteric symptoms continued. On the 29th, he took three ounces of olive oil, which seemed to remove the symptoms of dysentery; but nausea and vomiting supervened, and, on the evening of the 30th, he was attacked with a regular paroxysm of ague.

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* The newspaper in which the discovery of the effects of charcoal was first announced.

† See Littera sull' uso interno del Carbon di Legno nelle febbre intermittenti, page 11.

On the morning of the 31st, he complained of violent headach and weakness. The nausea and vomiting returned, and his appearance indicated considerable bodily disorder.

Sumat pulveris carbonis ℈i. statim, et vespere.

August 1. His appearance is much changed for the better. He has had no fever nor vomiting since yesterday morning; appetite returning; bowels perfectly regular; a great many eruptions have appeared on his lips.

Continuatur pulvis carbonis.

I am perfectly aware that the administration of charcoal powder internally is by no means new. I myself have seen it prescribed in the Royal Infirmary at Edinburgh, and it has been recommended by a number of foreign authors;* yet, as it has never been recommended with the intention herein set forth, our knowledge of these, its qualities, may be fairly considered as a discovery. But had its virtues been perfectly known before, its present neglect, contrasted with the foregoing evidence, is sufficient to justify me in urging it as strongly as possible upon the attention of the medical world.

Lazaretto, Leghorn, April 30, 1814.

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* Vide Moench, Thomannn, Horn, Brachet, Brugnatelli, &c. &c.

Composition of Eau Medicinale.

Two years ago Mr. Want discovered the composition of a medicine which possesses the power of removing the paroxysm of

Gout in a degree fully equal to the Eau Medicinale. Since that period he has had abundant experience to satisfy himself of the *identity* of the two medicines.

He directs a tincture to be made by infusing, for two or three days, a quantity of the fresh-sliced root of the *Colchicum autumnale*, in proof spirits of wine, in the proportion of four ounces of the former to eight of the latter. This tincture he employed in all his first experiments, but as the efficacious parts of the plant are soluble in water or wine, either of these menstrua may be used; and, to produce a medicine more particularly resembling the Eau Medicinale in its external circumstances, it is merely necessary to use good Sherry or Lisbon. Mr. W. purchased the root at Butler's, Covent Garden, but it may be procured in all the physical herb shops, and under the vulgar name of *meadow saffron* may be found in every part of England.

For medicinal purposes, a recent infusion of the fresh or dried root in water is equally efficacious. Mr. W. has made extensive trials with this watery infusion, and never been disappointed in its effects. He was led to employ the dried root, from observing its variable strength when fresh, in which it appears to be much influenced by the weather and season of the year. After rain, it contains a large quantity of water; but on the contrary, after much sunny weather, the watery parts of the plant are evaporated, and the active qualities more condensed.

The dose of this tincture, whether it be made with water, wine, or spirit, should be the same, and should vary according to the constitution of the patient. Upon an average, we may fix two drachms, or two ordinary tea-spoonfuls, as the proper quantity for an adult.

The wine of white hellebore has been supposed by some to be the French medicine. At a very early period of the promulgation of this opinion, Mr. W. spared no pains to ascertain how far it was founded in fact. He has employed hellebore in every possible form. In some cases it appeared to be possessed of efficacy; but a series of disappointments induced him to abandon it as a medicine on which no dependance can be placed. In its mode of operation it has some properties in common with colchicum, or meadow saffron, but in its power of curing gout, it falls infinitely short of it.

It is proper to state, that Mr. W's experiments have already been made in at least *forty cases*, followed by results of the most satisfactory nature, the paroxysms being always removed, and, in several instances, no return of the disease having taken place after an interval of several months.

Med. Phys. Journal, July, 1814.

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Mr. Want is authorized by Sir Joseph Banks, and Maj. Rennell, to publish their decided conviction that his medicine, an account of which was given in our last number, and the *eau medicinale* are the same, as far as they are enabled to judge from the appearance, taste, and smell. Several gentlemen conversant with the latter, who have seen Mr. Want's preparation at Sir Joseph's house, are of the same opinion.

Med. and Phys. Journal, Aug. 1814.

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New Remedy for the Itch.

Mons. Judelot has published a recipe for a sulphurated soap, which he conceives to possess peculiar advantages and facilities in the cure of the itch. He prepares it, by dissolving six ounces of pulverized sulphuset of potass in a third of its weight in water. Two pounds of white soap are then to be rasped and put into an earthen vessel in a water bath; to which are to be added, gradually, two pounds of almond oil, triturating it well with the soap as it is added. The solution of the sulphuret of potass is then to be put into a marble mortar, and well rubbed with the mixture of oil and soap, adding the latter very gradually. Two pounds of almond oil and two drachms of any agreeable essential oil are then to be added; and the mixture will be complete. This liniment should be kept in a close vessel, and used by rubbing an ounce of it upon different parts of the body particularly those affected, twice a day. It rarely fails to cure the itch in eight days. It possesses the advantages of having no diagreeable odour, of not irritating the skin, and of preserving the patient's linen uninjured. It is cheap, and may be preserved for almost any length of time. *Bulletin de la Faculte de Medicine.* *Ibidem.*

Plant producing Sugar.

M. Marsan, professor of medicine at Padua, has drawn up a memoir upon a plant more resembling the sugar cane in its botanical characters, and in the quantity and quality of the sugar which it yeilds, than any hitherto discovered. It is a large gramineous plant from the south of Africa described for the first time in 1775, by Peter Arduino, under the name of *Holcus cafer*, and well characterized by its velvet down and globular seeds. It is now cultivating in various parts of Italy, Bavaria, and Hungary.

Med. and Phys. Journal, Oct. 1814.

New Acid.—Gay Lussac, has finished a very laborious and complete investigation of the properties of *Iodine.* During his experiments he discovered that chlorine has the property of combining in two proportions with oxygen, and of forming two acids, which he calls the *chloric* and *chlorous* acids. The enchlorine of Davy, is Gay Lussac's *cholorus* acid, but it would seem that the *chloric* acid is the more curious and important compound.

Dr. Thompson's annals.

Tic Doloureux.—Mr. Kerrison has inserted in the Apothecaries' Journal, a case of tic doloureux, which, as it confirms the efficacy of a medicine recommended by Mr. Bailey, in this disease, we shall transcribe. It is to be observed, that Mr. Kerrison, on account of the success derived from belladonna in the instance here related, is induced, from analogy, to recommend its employment in any future case of hydrophobia which may occur.

In this case of tic doloureux, the nerve above the orbit had been completely divided without success. "One grain of the extract of belladonna was given in a pill, which was found to occasion vertigo and great lassitude, with a peculiar and distressing dryness of the tongue and fauces; but the pain was removed. It returned the next day, and was again kept in check by smaller doses of the same medicine. A quarter of a grain of the extract was given three times a day, and increased to a third of a grain; then a quarter of a grain morning and noon, and half a grain at night, sometimes omitting the medicines altogether for a day.

The pain, whenever it returned, was as certainly removed by the medicine. In the course of three weeks, the disposition to the *tic* gradually subsided, and the remedy was consequently discontinued. No derangement of arterial action was perceived during this guarded use of belladonna, nor were the bowels constipated. An augmentation of the dose was now and then attempted; but confused perception in a slight degree, and dryness of the mouth, always succeeded, although these effects subsided spontaneously in a few hours. The individual, with a mind of the highest order, and long habit of appreciating the effects of various medicines, was convinced of the power of this remedy over his complaint. Large doses of tincture of opium, and opium in substance, had previously been tried, without any good being obtained from them in this attack; and hemlock on a former occasion had also failed. The officinal solution of arsenic at that time, pushed to its full extent, had in some measure relieved, but not cured the disease; but it had been discontinued from its deleterious influence on the general system."

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A detachment of several hundred French troops having halted at a short distance from Pirne, they were allured by the inviting appearance of the berries of the Atropa Belladonna, which grew in abundance in the neighbourhood, to pluck and eat of them, whereby one hundred and eighty men became poisoned, many of whom died before professional assistance could be rendered them, and the rest were long in recovering. The following description of the symptoms is given by the medical officer who had the care of the troops in question. Dilatation and immobility of the pupil, total insensibility of the eye to the presence of external objects, or very confused and indistinct vision; conjunctiva turgid with purple coloured blood; prominence of the eye, which in some appeared dull and heavy, in others bright and furious; great dryness of the lips, tongue, palate and throat; delutition difficult, in some cases nearly impossible; nausea, not followed by vomiting; sense of weakness; lypothymia, syncope; inability to stand upright; bending forward of the trunk of the body; continual movement of the hands and fingers; lively delirium, accompanied with a silly laugh; aphonia, or inarticulate sounds uttered with difficulty; ineffectual inclination to intestinal evacuation; very gradual return to health and to reason, without any recollection of the preceeding state.

Failures in Vaccination.

The occurrence of small-pox after Vaccination has of late been too frequent and authenticated to leave any rational doubt of the fact. Whether such failures are not proportionally more numerous at the present time, than they have hitherto been, is a question deserving serious consideration, because, were this once ascertained, a well grounded suspicion might arise, how far the vaccine fluid had suffered alteration or deterioration in its passage through the various animal systems, previously to the insertion of it into the individual in whom the failure occurs. This view of the matter, seems to have been taken by Dr. Kinglake, in a paper in the last Medical Journal, who supposes, that the specific property of the vaccine virus is attenuated or exhausted in the changes it undergoes, by being indefinitely transferred from one individual to another. His arguments on this subject are at least specious, and he supports them by analogy drawn from the small-pox, the mildness of which, in its inoculated form, he attributes to the long continued succession of the practice, the transfer of the matter from one inoculated subject to another. Were this true, however, the small-pox would at length wear itself out and become extinct, an event of which there does not seem the remotest probability. It is well known also, that the violence of the disease, in any particular individual, does not in the smallest degree depend upon the *source* whence the infection was derived, whether from a person in the natural, or in the inoculated small-pox, but that it is influenced by the constitutional state of the person who receives the infection, and by the mode in which it is communicated. Thus a person *inoculated* from a pustule of the most virulent case, will often pass through the disease mildly, whilst one who receives the infection in the natural way from the mildest inoculated small-pox, will undergo a malignant and even fatal disease. Notwithstanding we ourselves are not convinced by Dr. Kinglake's reasoning on this intricate law of the animal œconomy, we shall subjoin an extract from that part of the paper, where his opinion is fully stated.

"My opinion on this subject is, that the vaccine fluid is too long used in the human system without renovating it from the cow, which, being its genuine source, must be supposed to yield it in its most efficient specific powers. Why then not frequently resort to the animal that, by a morbid action peculiar to its nature, furnishes the desired preservative? The dairy districts are often affording natural instances of the disease; and if this opportunity did not offer with sufficient regularity, would it not be practicable always to reserve a portion of the immediate vaccine fluid with which to infect a cow, so that it may be thus genuinely supplied, as occasion may require? Does vaccination directly from the cow produce a more violent disease, than when it is transferred from one subject to another? If so, the security against small pox would probably, be proportionably great, and yet the higher degree of the disease would not be accompanied by any calculable risk of life. The disease, as contracted from the infected cow is, I believe, usually attended with general eruptions over the skin; but to my knowledge it is not on record, that it has in any instance proved mortal. This being the case, it behoves, in my judgement, the advocates for the real benefit and permanent reputation of vaccine inoculation, to go to the cow very often for a fresh supply of genuine fluid, in which could not fail to be found those unaltered specific powers, on which its preventive efficacy against small-pox depends. Either recourse should be had to the cow in every instance, or, consistently with the motive for doing so, the same matter should be re-inoculated, perhaps, more than six times. To that extent, no diminution of power may, probably, be sustained; but this is a question that can only be absolutely set to rest, by uniformly recurring to the animal, that has originated the morbid fluid. That the vaccine fluid undergoes some change by long continued inoculation, derives support from the notorious fact, that, since comparative discontinuance of inoculation for the small-pox, the variolous fluid, from being almost entirely furnished by natural cases, is found on being employed, in inoculation, to induce much more virulent disease, than when the frequency of the practice caused it to be transferred from one inoculated subject to another, by which the matter was probably rendered more mild than when it resulted from the natural form of the disease, so as to impart to the inoculated small-pox a morbid character almost free from calculable risk. This is a reason, why a partial indulgence of inoculation for the small-pox becomes a public evil, by subjecting to hazard the lives of those who, under a more general practice, would have to undergo a comparatively mild and safe form of the disease. The prohibition of variolous inoculation, therefore, by a suitable legal enactment, would be imposing a wise, humane, and beneficial restraint, which, aided by the anti-variolous efficacy of the vaccine disease, would afford a reasonable prospect, at no distant time, of altogether exterminating the small pox. Inoculation for the small-pox keeps the disease always in action, and by subjecting persons to a casual infection, has proved rather a scourge than a benefit to mankind, in as far as the deaths resulting from the frequency of the natural disease, owing to inoculation, are held to have exceeded the average amount of mortality from that disease, occurring for any given period, previously to the introduction of variolous inoculation."

Another gentleman, in the same Journal, Mr. R. Walker of Oxford, whilst he admits the occasional failure of vaccination in preventing small-pox, attributes the failure to a very different principle; he does not appear to consider the matter as at all deteriorated in its progress through the human system, since he very justly observes, it assimilates a certain portion of the fluids circulating in the system to its own nature, but to a defective action of the virus arising from the actual state of the system into which the virus is introduced. The view of Mr. Walker on this subject has something novel in it, and we here present it to our readers.

"The *cow-pox* I consider as a different modification only of the *small-pox*; and though it may be, *ordinarily*, considered as a security against that disease, is yet less certain than the *small-pox* itself. The circumstances of the *small-pox* being incident to every person, and ordinarily, *once only*, and the greatest probability of an attack of the *small-pox* after the *cow-pox*, than a second attack of the former, I would attempt to account for thus. I consider every person as having the essence or innate principle of the small-pox originally in the system, which remains *dormant*, unless matured and put into action by an exciting cause, of which, *infection* is the usual one; and to which every individual is so liable to be exposed, that no one has a reasonable expectation of escaping it.

Whenever the essence or innate principle of the disease is by any means put into action, it assimilates to its own nature a certain portion of the fluids, circulating in the system, more or less according to the grossness or purity of the habit, and throws out or expels them. If the whole of the original essence be expelled, or the system entirely purified of the disposition, which is ordinarily the case, the person will never be subject to a second attack. The more complete the ferment or commotion in the system may have been at the time of the disease, the greater will be the probability of the whole having been thrown off, and *vice versa*. Hence, although the cow-pox, properly passed through, may be a security *ordinarily* against the small-pox; yet, considering that persons pass through this disease, for the most part, without any eruption, and scarcely any discernable ferment or commotion in the system, I should be less surprised at an attack of the small-pox after the cow-pox, than a *second attack* of the small-pox itself."

Mr. Walker thinks it perfectly accordant with his principle, that a recurrence of the *natural small-pox* is an exceedingly rare occurrence; that natural small-pox happening subsequently to inoculated small-pox, is less rare, and that the occurrence of natural small-pox after vaccine inoculation, is still less rare. Upon what data the following statement is formed, we are not acquainted, and have no means of judging of its accuracy. "I think I may venture to consider the following statement as nearly correct, or at least perfectly within the limits, viz. a recurrence of the natural small-pox once in ten million cases; natural small-pox, occuring after inoculated small-pox, once in one million cases; and of natural small-pox after vaccine inoculation, once in ten thousand cases." *New Med. and Phys. Journal, Oct.* 1814.

Unguentum Hydrargyri.

A correspondent in the Apothecaries' Journal states, that he has discovered a new mode of preparing the strong mercurial ointment, which considerably lessens the labour and time usually required for its preparation. This discovery, the result of the carelessness of the author, when a boy, who having left a portion of mercurial ointment in a mortar, after a while set about making some more, without having cleaned out the mortar. He found that the quicksilver was much more easily extinguished than if the recent lard had been employed; and attributes the difference in the process to the addition of the old ointment. We apprehend the author is correct as to the fact of the facility of the combination produced by adding the old ointment, but presume he is mistaken if he attributes any share of the effects to the mercury already in combination, (otherwise than as the viscidity of the ointment may be rather greater) the whole of which seem to belong to the rancidity of the lard contained in the old ointment. We very well know, that a small quantity of old rancid lard will greatly facilitate the incorporation of quicksilver with the recent lard to be employed in forming the ointment, although we are far from recommending this mode, and prefer ointment made with the mildest possible fat and a greater degree of trituration. Indeed, it is contrary to all analogy that two substances will combine the more readily for having a quantity of the combined product in a state of maximum added. Water will dissolve a certain quantity of nitre, and with a certain degree of readiness; nor will the combination be at all assisted by previously adding a portion of saturated solution to the materials. Were the author's explanation of the fact true, we should expect to find that balsam of Copaiba would be more readily rubbed down with a mixture of balsam and egg, than by egg alone; that oil would be rendered more miscible with water by a previous mixture of oil and mucilage, than by mucilage alone; in short, that the solubility of any given substance is increased in exact proportion to the quantity of that substance already in combination with the solvent. *ibid.*

DOMESTIC INTELLIGENCE.

On the late epidemic fever in Boston, and elsewhere.

DURING the summer and autumn, fever has prevailed more extensively in this town than during many years past; and the same has been noticed in various parts of the country not far distant, though not in all the towns in the immediate vicinity. Some cases were seen in the spring, they began to be frequent in July, and gradually became more so until November. Since the middle of that month, the disease has been rapidly disappearing.

This disease has been denominated *typhus*. For two or three years past this name has been getting into vulgar use among us, and that not without producing much respectful terror among the people. Our epidemic has been the *simple continued fever* of Fordyce, in far the greatest number of cases. In some instances the fever was combined with general inflammation. The proportion of such instances was small during the autumn; in the summer it was more considerable. In these cases epistaxis was a common symptom without affording much relief.

In the majority of cases which came under our inspection, the disease affected every part of the system as equally as fever is ever found to do. In a few cases some symptoms of putridity shew themselves. The disease was rarely protracted beyond the third week, and generally was shorter in proportion as medical assistance was afforded early or late. The period of convalescence was long when the disease went through its whole course without having been much mitigated by medicine; but short when early attention was given to it.

The instances of death from this epidemic were rare; perhaps the whole number of deaths during its prevalence was not much, if any greater than is usual at the same season in other years, when the number of sick has been much less. A tendency to diarrhœa was among the most unpleasant circumstances, occasionally seeming to protract the disease, and in more than one instance to render the event of it fatal. In the early period of the disease the remissions in the morning were sometimes very perfect. In these cases the patients were misled, and believed themselves well. Errors of conduct founded on this beleif not only increased the succeeding paroxysms, but in some instances occasioned the disease to be more severe and longer than it would otherwise have been.

Powerful emetics were highly beneficial, if administered at an early period of the disease. When exhibited within the first four days they seldom failed either to arrest its course at once, or so to mitigate its character that it gradually declined in the course of a few days. When mild emetics were given, or when the operation was not severe, the benefit was much less. The benefits of emetics were rendered more certain when they operated freely on the bowels, or were followed by active cathartics. At whatever period of the disease the medical treatment was commenced, it was thought useful to begin this with an emetic; and where circumstances led to the omission of this remedy, this omission was afterwards regretted.

After the evacuations, when these did not occasion the disease to terminate, the use of antimonials was principally relied on. In a few cases only, and at an early period, we found benefit from combining preparations of quicksilver with those of antimony. In the use of the tartrite of antimony we followed the method of Odier, published in the third volume of this Journal, (page 248.) and we were surprised to find patients bearing a grain, and even two grains of that medicine, once in two hours through several days, not only without nausea, but without any very sensible effects on the bowels or skin. In some instances, however, the effects on the bowels were very powerful, making it necessary to diminish the doses and to counteract those effects by opium. The benefits derived from this medicine were greatest when it did not occasion considerable evacuation of any kind. These benefits were greater the earlier in the disease the remedy was employed. It was useful when not administered until the second week, but after this period the course of the disease could not be much affected by art.

After the first evacuations, it was sufficient to maintain the regular alvine discharges; for purgatives produced a debility, which rendered it necessary to employ them with caution. Yet in one or two instances, where it was evident that an accumulation of offensive matters had taken place in the stomach and bowels, emetics and active cathartics were administered at a late period of the disease with unequivocal advantage. Vesication of the abdomen and of the head was found very serviceable in severe cases; the former especially, when the bowels were distended by air, and when the diarrhæa was urgent.

Messrs. Bradford & Read, of this town, by advertisement on the covers of this Journal, have announced to the public, their determination to publish the Dissertations on Fever, of the late George Fordyce, M. D. &c. &c.

The reputation of the work now first proposed for publication in America, is too well established to be enhanced by any observations we might be disposed to make. Its publication among us must promote the diffusion of the most correct principles on the subject of fever.

This work is remarkable for the perpetual novelty which it offers those who study it. It is not sufficient to have perused it once. It must be kept on one's own shelf. It must be read with more than ordinary attention in order to be relished, or understood. It is one of its peculiarities that it imposes on the student of medicine no common exercise of his mind. It is wholly the result of observation; of observation made under the most favorable circumstances, on every variety of disease, with whose character, if not with whose nature, it professes to acquaint us. Since the days of Fordyce, the practice in fever has undergone some change. Articles of the materia medica, possessing different properties, have been substituted for those employed by this physician. We do not stop to inquire what benefits have followed this medical change: but it is a fact, that in the hands of respectable practitioners the old remedies still hold their place, and perhaps the result is as favorable as that of the most complicated practice. But it is the facts which this book contains, and the general ideas which they gave birth to, those principles in short which they have established, on which the reputation of its author rests; and it is a basis so broad, so firm, that perhaps little short of a perfect revolution in the animal structure, certainly in the signs of its morbid actions can undermine or destroy it. But more particularly it is in the discriminations between fever and diseases which resemble it, in the analysis and arrangement of the varied phenomena of fever, and of their complication, of the combination of fever with other morbid affections, and of the occasional irregularities of the disease: it is in these important circumstances, that Fordyce justly challenges the foremost rank, among the numerous writers on fever.

We might further notice the precision and the fixedness of mind with which the author has contemplated his subject, in all its relations. No unnecessary or foreign disquisitions divert him from the great object of his research; and as fact is with him, his only authority, he treats mere speculation and forced analogy, with that indifference, or severity, they justly deserve.

It is with great pleasure, we observe, that whilst *manuals of practice*, and physician's *vade mecum's*, issue daily from the press, that there is a demand for a work of a character so strictly philosophical as the one under notice. It augurs well of the medical atmosphere which we breathe, and we indulge some good assurance that the spirited publishers of this work, even at a moment in some views unpropitious, will not find their undertaking unrewarded.

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Cicuta Maculata.

Since the account in our last of the poisonous effects of this plant, we have seen in the Medical Repository another instance of its fatal consequences, in the case of some children who eat of it by mistake, for the sweet flag-root.

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The following European Works have been recently published.

Letters addressed to his Royal Highness the Duke of Kent, on Consumption; containing Remarks on the Efficacy of Equable and Artificial Temperature in the Treatment of that Disease, &c. By Thomas Sutton, M. D. of the Royal College of Physicians, late Physician to the Forces, and Consulting Physician to the Kent Dispensary; and author of Tracts on Delirium Tremens, Gout, &c.---Price 2s. 6d.

Practical Observations on Insanity; in which some Suggestions are offered towards an Improved Mode of Treating Diseases of the Mind; and some Rules proposed which may lead to a more humane and successful Method of Cure; with Remarks on Medical Jurisprudence, as it relates to Diseased Intellect. By Joseph Mason Cox, M. D. Price 8s. boards, 8vo.

An Essay on Bronchitis; with a Supplement containing Remarks on Simple Pulmonary Abscess, &c. &c. By Charles Badham, M. D. Second Edition, corrected and enlarged, boards, 5s. 6d. 12mo.

Elements of Medical Jurisprudence, or a succinct and compendious description of such tokens in the Human Body as are requisite to determine the judgment of a Coroner, and of Courts of Law in cases of Divorce, Rape, Murder, &c. To which are added, Directions for preserving the Public Health. By Samuel Farr, M. D. Second Edition, 12mo. boards, 5s. 6d.

A Treatise on the Antiphthisical Properties of the Gratiola Officinalis, or Hedge Hyssop, with Directions for its use in Pulmonary Consumptions and Asthmatic Complaints. By Richard Reece, M. D. Member of the Royal College of Surgeons, Author of the Medical Guide, &c. &c. Price 2s.

Commentaries on the Treatment of the Venereal Disease; particularly in its exasperated state, containing Advice relative to the Use of Mercury, so as to insure its successful effect: with an Appendix, recommending an Improvement in the Treatment of Strictures. By Edward Georghegan, Member of the Royal College of Surgeons in Ireland, &c. &c. 8vo. Price 6s. 6d. boards.

A Treatise on the Gout, containing the opinions of the most celebrated ancient and modern Physicians on that disease, with observations on the Eau Medicinale. By John Ring, Member of the Royal College of Surgeons in London. 8vo. 16s.

Observations on Pulmonary Consumptions. By Henry Helbert Southey, M. D. 8vo. 7s.

Practical Observations on the best mode of curing Strictures in the Urethra, with Remarks on the inefficacy and ill effects of Caustic applications. By William Wadd, Member of the Royal College of Surgeons, London. New edition. 8vo. 4s.

Facts and Observations deduced from long and extensive practice in Liver Complaints and Bilious Disorders in general, and on such derangement of these Organs as influence the Biliary Secretion, with some new and practical observations on the various appearances of this important Secretion, connected by an appropriate and successful mode of Treatment, and the whole illustrated and confirmed by a numerous list of cases. By John Faithorn, formerly Surgeon in the East India Company's service. 8vo. 5s.

A Manual of Mineralogy. By Arthur Aikin, Secretary to the Geological Society. 8vo. 4s.

A Treatise on the Hereditary Properties of Diseases, intended to remove unfounded errors and ill-judged cautions consequent on erroneous opinions; with Notes illustrative of the subject, particularly Madness, Scrofula, and Cutaneous Diseases. By Joseph Adams, M. D. F. L. S. &c. 8vo. 5s. 6d.

An Inquiry into the History and Nature of the Disease produced in the Human Constitution by the use of Mercury, with Observations on its connection with Lues Venerea. By Andrew Mathias, Surgeon-extraordinary to the Queen, &c. Second edition. 8vo. 7s.

Flora Americæ Septentrionalis: or a Systematic Arrangement and Description of the Plants of North America; containing, besides what have been described by preceding Authors, many new and rare species, collected during Twelve Years Travels and Residence in that Country. By Frederick Pursh. 2 vols. 8vo. 1l. 16s. With coloured Plates, 2l. 2s. 6d.

An Account of the most important recent Discoveries and Improvements in Chemistry and Mineralogy, to the present time; being an Appendix to their Dictionary of Chemistry and Mineralogy; by A. & C. R. Aikin. 4to. 18s.

Essay on the Theory of the Earth. By M. Cuvier. Translated from the French, by R. Kerr, F. R. S. with Mineralogical Notes, by Professor Jameson. 8vo. 8s.

Veterinary Medicine and Therapeutics; containing the Effects of Medicines on various Animals; the Symptoms, Causes, and Treatment of Diseases, with a select Collection of Formulæ. Part I. The Materia Medica, Pharmaceutical Preparations and Compositions. Part II. The Disorders incident to Neat Cattle, arranged according to the Nosology of Cullen. By W. Peck, London. 10s. 6d.

A Treatise on Hydrocephalus, or Dropsy of the Brain. By James Carmichael Smyth, M. D. &c. 6s.

Lectures on Comparative Anatomy, with 132 Engravings. By Sir Everard Home, Bart. F. R. S. 2 vols. royal 4to. 7l. 7s. 2 vols. imperial 4to. 10l. 10s.

TO CORRESPONDENTS

IT has always been an object of the New-England Journal to present to its readers from time to time the latest foreign medical intelligence. How far this purpose has been accomplished during the present embarrassed state of literary intercourse, the readers of the work must judge. In the present Number they will find extracts from the different English periodical publications down to October, 1814, the last of which were received a few days since.

Upon entering on their fourth volume, the editors derive much encouragement from the increasing subscription and liberal support of the work. They have reason to believe that its present circulation is not exceeded by that of any similar publication in the United States. It may be proper to add, that the circulation of its materials is still more extensive. Correspondents who make the N. England Journal the medium of their communications should be apprized that as a majority of its original papers have hitherto been regularly copied into the London Journals, their productions may be presented to the faculty in all parts of the world where the English language is spoken.

A number of Communications received during the past year, are necessarily omitted, principally from the inconvenient length of their theoretical speculations.

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ON THE USE OF CHARCOAL, AS A SUBSTITUTE FOR PERUVIAN BARK.

BY DR. MAKESY, *Surgeon of the 62d British Regiment.*

[For the New England Journal of Medicine, &c.]

AMID the jarring collision of political contention and national controversy while the loud, harsh voice of hostility too frequently tends to paralize the activity of scientific research, and to suppress the feeble tones of human sufferings; any attempt to promote the interests of humanity, unbiassed by the frowning aspect of existing circumstances, it is presumed, will not be unacceptable to an enlightened and liberal mind.

I, therefore, beg leave to solicit your attention, and that of your professional brethren, to a remedy, lately used with marked advantage in Sicily, and other parts of the Mediterranean in intermittent and remittent fevers. The medicine alluded to is the carbo ligni or common charcoal properly prepared; and a numerous series of accurate trials there, both in British Hospitals and among the inhabitants, support the opinion, that it may prove an excellent succedaneum for the cinchona, in most instances where that remedy is deemed applicable.

Indeed there appears to exist a striking analogy between the charcoal powder and Peruvian bark, with respect to their effects on the system. They both frequently purge in the beginning, while constipation follows their continued use. The charcoal, like the bark, promotes appetite, adds tone to a languid system, prevents the recurrence of febrile paroxysms, and corrects or removes a tendency to morbid associations. Bark retards the chemical decomposition of organic substances; charcoal is one of the most powerful antiseptics known. Under peculiar circumstances the charcoal would appear to claim a superiority, particularly in the periodical fevers of warm climates, which are so frequently complicated with visceral affections or an inflammatory diathesis, where the bark proves frequently useless, sometimes injurious. In the advanced stages of the bilious remittent fever, and when the primary excitement is followed by extreme debility, the use of charcoal has had excellent effect, and although frequently given in a very irritable state of the stomach, it seldom produces nausea or vomiting.

The knowledge of the sanative virtues of charcoal has already diffused its beneficial effects among the Sicilian poor, to whom the use of bark from its high price is frequently denied.

In a village called Aliminusa, some distance from Palermo, situated on a plain in the vicinity of marshy ground, the inhabitants are annually much subject to agues. The properties of charcoal being made known there through the medium of a newspaper, they had immediate recourse to its use, and the medical practitioner of that district reported, that one hundred and five cases were speedily cured.

I am perfectly aware, that the use of carbonaceous substances is of a very ancient date. Hippocrates recommends burnt sponge in chronic menorrhagia, (*Vide de Morbis Mulier.*) Their use is likewise favourably mentioned by de Haën, Moench, Thoman, Honn, and other continental authors. But the idea of the exhibition of charcoal in intermittents, originated with a young student of medicine in the chief Hospital of Palermo, who, reasoning from some crude notions of chemical affinity, proposed it as an internal remedy, combined with dilated sulphuric acid. It was exhibited, thus combined, in two cases of intermittent, then in the city Hospital, with speedy success. The favourable result of these cases being communicated to Dr. Calvert, Physician to the Forces, he made a similar trial of it in some cases under his treatment, without the sulphuric acid, and his report induced Dr. Borland, the chief medical officer of the British forces in the Mediterranean, to direct the attention of the medical officers of that division of the army, to a subject so new and interesting. Many cases presented themselves in the numerous Hospitals, for the trial of the effects of charcoal in intermittents and remittents, and several reports of its success are now on record; two or three doses being often found sufficient to interrupt the expected paroxysm, and cut short the disease.

An essay, entitled "A Treatise on the Internal Use of Charcoal in Intermittent and Remittent Fevers, written by the Surgeon of the 62d regiment," has been translated into Italian, and published in Palermo; it has likewise been transmitted for publication in the Medico-Chirurgical translations of London, accompanied by other interesting reports on the same subject, and is most probably now in circulation.

The intended limits of this communication forbid minute detail of the advantages obtained from the use of charcoal in dyspepsia, in protracted cases of diarrhæa and chronic dysentery, combined with affections of the liver. It has likewise been given with benefit in scrophulous sores, and foul sloughing ulcers, applied externally and taken internally; it shortly corrects all putrid fetor, and forwards the formation of healthy granulations.

To obtain the desirable results from the use of charcoal, the best and lightest pieces ought to be selected, coarsely broken, and kept about fifteen minutes in a red heat in a covered vessel, then taken from the fire, any ashes on the surface removed, and speedily reduced to a very fine powder, and kept in a bottle well stopt. The dose of charcoal, like all other medicines, depends on the circumstances of a given case; but from half a drachm to two drachms given three or four times a day, during the intermission, or immediately before the expected paroxysm, will in most cases prove sufficient. It may be taken diffused in water or any other convenient vehicle. On the dose being swallowed, a slight sensation of warmth is usually felt in the epigastrium, not unlike that produced by taking a small portion of pure spirit. It seems to promote all the secretions, often producing at first some bilious alvine evacuation; but costiveness in general follows its continued use.

The foregoing remarks are stated with a confidence, which is the offspring of reiterated observations, and under circumstances which preclude the probability of mistake. However, as the subject is new and still demands the illustration and support of a more enlarged experience, I feel gratified in being the first to invite the attention of the medical gentlemen of America, to a remedy which future trials may deem extensively useful.*

I remain,

your obedient servant,

A British Medical Officer.

Castine, Jan. 1st, 1815.

A CASE OF PUERPERAL CONVULSIONS.

COMMUNICATED BY JOSIAH BARTLETT, M. D.

[For the New England Journal of Medicine, &c.]

Labours, preceded by, or attended with convulsions, have been justly classed, by writers on obstetricks, among the most alarming occurrences in that branch of the medical profession, and the rules for conducting such cases are said "to have been founded on less certain principles, and to have been less confirmed by experience, than those which have been given for any other cases that occur." I believe from the few instances of this kind which have occurred in my practice, during a period of thirty-three years, that puerperal convulsions are less frequent here than in Europe; and it may therefore be deemed useful, especially to those who are unable to consult the various authorities, and may be called to a prompt decision in periods of great emergency, to relate the occurrences of a striking and important case of this kind.

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* The editors cannot omit praising the excellent spirit, which dictated the communication of the above remarks, at a time when the author, with his fellow soldiers, was daily expecting o be assailed by those, for whose benefit he wrote.

On the 25th of January, 1815, at nine P. M. I was called to Mrs. M. B. aged twenty-three, a woman of a robust habit, whom I attended in her first parturition (which was not accompanied with any thing unusual) on the 28th of August, 1812. She was advanced in the ninth month of pregnancy, had been in perfect health, with the exception of a slight diarrhæa for a short time; though, about ten days before, she accidentally fell on a painted floor, which produced no apparent injury. She was now in a strong convulsion fit. I found on inquiry, that her face and lower extremities had been swollen; there had been an appearance of fulness in the vessels; she had taken dinner as usual, and a light meal at evening; previous to the attack she complained of dizziness, and at the time had an evacuation from the stomach. Her pulse was quick and very hard, respiration laborious, and the spasms violent. I took twenty ounces of blood from her arm, and forced down a large dose of *tinctura opii*. The fits continued with increasing strength and duration through the night, in which, the medicine (which she once ejected) was repeated, and sixteen ounces more of blood were taken without any visible effect.

26th. After repeated examinations, I discovered, about six o'clock, A. M. that the os uteri began to dilate, and that there were some efforts resembling labour, during the remission of the fits. I now administered an enema, and endeavoured to assist gradually in dilating the parts. At eight o'clock, the membranes protruded into the vagina, and the head of the fœtus rested upon the brim of the pelvis. The number of fits to this period had been twenty-four, her pulse was sinking, and a fatal event expected. I therefore determined, in concurrence with the opinion of Doctor A. B. Thompson, who had been called in consultation, to attempt delivery as speedily as possible. To effect this, the membranes were ruptured, and the crotchet, guarded by the left hand, was introduced, in such manner as to obtain a firm hold of the child, and I effected the extraction with ease. The placenta followed without hæmorrhage, she was laid in bed, and the tinctura opii was freely used during the day. The fits continued with the same violence, but less frequently, until four o'clock, P. M. to which time she had fifteen. An application of humulus lupulus with alcohol dilutum, was now made to the abdomen, and of cataplasma sinapeos to the back of the neck, and soles of the feet. At eight she appeared to sleep, and directions were given not to interrupt her.

27th. At eight, A. M.—She had slept several hours, but at intervals was restless; two smaller doses of tinctura opii were given during the night, but no nourishment was taken. She had discharged urine, had a small evacuation by the rectum, the lochia were moderate, and there was no heat nor tumefaction of the abdomen. I now, for the first time from the attack, discovered symptoms of consciousness, (for during the suspension of all the paroxysms she was in a profound coma) and gave her a table-spoon full of oat-meal gruel. At one o'clock, P. M. she took a dose of ol. ricini, at three knew me and answered questions. In the evening I directed a repetition of the purgative medicine, to be followed by tinctura opii, if she was restless.

28th. At eight, A. M.—She had a restless night, and no evacuation from the bowels, though she appeared much better, and was able to converse. I directed an enema, but a copious effect from the ol. ricini prevented its use. She bore liquid nourishment, with wine, through the day, was taken out of bed, and had her clothes changed without much fatigue; her mind was gradually recovering. The pulse from the time of the last fit was from one hundred and twenty to one hundred in a minute. In the evening directed a moderate dose of tinctura opii.

29th. At nine, A. M.—She had but little sleep, and was restless during the night, was perfectly rational but disposed to be talkative. There was a slight degree of heat, and I directed spt. æth. nitr. to be taken occasionally. She took less nourishment than on the preceding day, but was evidently gaining strength. At evening was affected with flatus in the stomach and a slight diarrhæa; the lochial discharge was regular, and no tenderness on the abdomen. I directed papaver somniferum in the usual quantity to be repeated, if necessary to procure sleep.

30th. At ten, A. M.—She had a refreshing night. In the course of the day, she sat in a chair half an hour, and took frequently of chicken broth. The milk is secreted without difficulty, and she is evidently on the recovery.

31st. At ten, A. M.—She slept well, her first passages regular, she took a small quantity of animal food without any injury, and her appearances have assumed those of a common case of parturition.

ON THE USE OF ARSENIC IN CANCEROUS COMPLAINTS.

BY THOMAS SEWALL, M. D.

[For the New England Journal of Medicine, &c.]

The celebrated empiric, Peter Davison, whose pretensions to the cure of cancers have of late excited so much interest in the minds of the community in this part of the country, has recently visited this town. He has here had a large number of applicants labouring under cancerous complaints, some of which were well defined, and had long resisted the ordinary modes of treatment. A number of these, he has treated with complete success; and nothing but time is wanting to prove, that these cures are radical.

He applies his remedy to the cancer in the form of a plaster, which excites great pain, inflammation, and swelling, and appears to possess the remarkable power of destroying the vitality of the whole of the schirrus, or cancerous part. This soon sloughs off, leaving nothing but a simple ulcer, which is easily healed by the ordinary means.

I have now the pleasure to inform you, that by a number of experiments, I believe, I have detected the composition of his plaster, and find it to contain *arsenic*, which, I have good reason to suppose, is its only active ingredient.

This is disguised by the admixture of a large proportion of some vegetable powder. The composition is generally mixed with the yolk of an egg, spread on pieces of fine linen proportioned to the size and form of the diseased part, to which it is confined by strips of adhesive plaster extending some way over the sound skin.

I am now using arsenic in cases of cancer in the same form, in which he applies it, and with a degree of success equally flattering.

I prepared my plaster with half a drachm of white arsenic levigated and mixed with the yolk of an egg, a little honey, and a sufficient quantity of flour to form the whole into a paste of a proper consistence to be spread on linen. Probably a much smaller proportion of arsenic would have answered equally well.

I am inclined to think this form of applying arsenic to cancers is the most eligible, that has hitherto been practised. Its action is here powerful, uniform, and constant; advantages, which the powder and solution of this mineral do not combine.

A number of Davison's patients were situated, where I had frequent opportunities of inspecting them while under the operation of his medicine, and I have seen nothing but the effect of the external application of arsenic carried to an unexampled extent.

It will at once be recollected, that the use of arsenic in the treatment of cancers is no new practice. It has formed the base of nearly all the quack remedies, that have ever been brought before the public. The celebrated cancer powders and washes of Plenket, Guy, Martin, and Pope, were indebted to it for all their activity and success.

But the use of arsenic has not been confined to the hands of quacks, regular physicians have long considered it a valuable acquisition to the materia medica, and a powerful remedy for some of the most formidable diseases.

Doctor Fisher has for many years been in the habit of applying the powder and solutions of this mineral to cancerous and scrophulous ulcers, with the most happy effect. In his hands, it has always corrected the discharge of such sores, and often effected a separation of the diseased from the sound part, and thereby performed permanent cures.

The frequent failure of arsenic in the treatment of cancers, while in the hands of regular practitioners, I apprehend, is to be ascribed generally to some defect in the mode of its application, to its indiscriminate use in all cases, to a want of confidence in the patients, but especially to a want of boldness, energy, and perseverance in the practitioner.

Ere long I hope to be enabled to lay before you a history of a number of well attested cases of cancer cured by arsenic, together with a view of the discriminating marks of such cases as warrant its application.

Ipswich, December 16, 1814.

Note.—Mr. Peter Davison appears to be acting a part, which has already been performed with equal success by hundreds of others of the same fraternity, and his medicine like that of his predecessors will, no doubt, meet with the same fate. The external use of oxide of arsenic, as Dr. Sewall observes, is not a new remedy in cancer. Its power over this complaint as well as those of a scrophulous nature, has been amply investigated by Hahnmann, le Febure, Justamond, Plenck, Monro, and many others. If these men have considered it as a safe and effectual remedy, in this disease, how comes it that its use has been and is now confined to the empyrics? The physicians and surgeons of the present day cannot, with justice, be accused of timidity in practice; and if the conclusions of the above mentioned authors were favourable to the application of arsenic in the treatment of cancer, we doubt not that it would have been adopted with avidity by regular practitioners. But do these authors recommend the adoption, or even speak in civil terms of this mineral poison? On the contrary, Plenck observes, that he has known many unfortunate accidents to arise from its external application. Monro says, he has cured some tumours, which were supposed to have been scrophulous or steatomatous, by it; but that the *true cancerous tumours*, which had been removed by its means, were reproduced, like those that had been extirpated by the knife; and he believes that we have no adequate motive for persisting in the use of so dangerous a remedy. Dr. Monro applied the arsenic in the same way as that described by Dr. Sewall, viz. mixed uniformly with the yolks of eggs. The French authors alluded to, speak of the danger to the system, arising from the absorption of arsenic. Brugnatelli, who has seen the bad effects of this substance, emphatically condemns, in his general pharmacopœia, its use, both internally and externally. The late experiments of Mr. Brodie also prove, that arsenic when externally applied, at least to a denuded surface, is capable of being absorbed and of producing in the system, the same train of symptoms as when it is taken into the stomach.

In fact, this substance as a local remedy, does not appear to have any specific power over the disease. Any other caustic, equally powerful, would probably produce the same effects. When the tumour is small and the neighbouring parts have not participated in the diseased action, arsenic destroys its vitality, the dead part separates and suppuration and healthy granulations supervene. Where the situation will allow of it, this state of the part may be produced much more expeditiously and with much less protracted pain by the knife. In those cases, in which the tumour is extensive, for example, extending from the breast to the glands in the axilla, and in those in which considerable ulceration has taken place, the use of arsenic without taking into consideration the possibility of its producing its specific effects on the system, will be accompanied with excessive pain, without being followed by any permanent relief. We say permanent relief; because, in the ulcerated state of this disease, there is little doubt, that the direct application of this caustic, will often at first lessen the fœtor, correct the discharge, and cause the surface to assume a healthier appearance. Even the size of the ulcer for a time gradually diminishes, and the patient has a flattering prospect of full restoration to health. But these good effects are not produced often without expence to the constitution; for they are purchased with great pain and suffering, and symptoms of general irritation appear in the system. The same consequences likewise follow the use of the narcotics, and particularly of the leaves and extract of the *conium maculatum.* In fact, as a palliative we are inclined to give the preference to the latter. The good effects of both, however, are but temporary, and they only lengthen the duration, and for a time diminish the horrors, by inspiring hope, of this terrible disease.

The celebrity which arsenic has acquired as a remedy in cancer, appears to have arisen from its indiscriminate application to tumours of all descriptions. From the ignorance of the patient, no distinction can be expected; and from the quack, to whom he applies, and whose decision is to be biassed by his interest, no sincerity, presuming on his knowledge, can be hoped. If true to his craft, all indolent tumours, and all old, ill conditioned ulcers, form but one class named cancer. He applies his arsenic; the patient, after some weeks of suffering, both mental and corporeal, is cured; the latter, of course, magnifies the extent of his dangers, and the former boasts of his skill and judgment, being well aware that the reputation of curing one cancer and having one living witness, will outweigh, in the public mind, the unsuccessful treatment of a dozen, who have been consigned to the silent tomb. We do not doubt that the cases which have given celebrity to any cancer-curer, were those, which were not cancerous, and which, if cancerous, might have been cured in a shorter time by the use of the knife. How many of the cases in Ipswich, cured by Mr. Peter Davison, were of this description, we know not. Dr. Sewall is a judicious practitioner, and we presume will give more time to the consideration of the nature of the cases in question, before he decides on the utility of a medicine, which has already been extensively employed and been found inadequate to the radical cure of genuine cancer. We must confess it surprised us to find, that the village of Ipswich should furnish, not only Mr. Peter Davison with "a great number of cases," but Dr. Sewall himself, with "several cases" of cancer, when the disease is so rarely seen in Boston.—Editors.

REMARKS ON DR. BOWEN'S CASE OF FUNGUS HÆMATODES.

[In a letter to one of the Editors.]

I have been much interested with Dr. Bowen's account of a diseased arm, given in your New-England Medical and Surgical Journal for the last month. But I am sorry to have to differ from the ingenious author as to the nature of the disease. Mr. Hey's account of symptoms and his descriptions of the *Fungus Hæmatodes* seem not to agree with it, as given in the American edition of his work ; (for I have not seen the 2d English edition.) But the structure and contents of the tumors agree much better with the description of *carcinoma* by Dr. Adams. The sac ; the cancellated structure of the interior ; the gelatinous nature of part of the contents ; the interspersed cartilaginous substances, with the spiculæ of bone ; the repetition of the sacs (as Dr. Adams would suppose by generation) ; the enlarged appearance of the glands ; the extenuation of the muscular fibres ; the absence of bleeding and blood ; and the connexion of the diseased parts with the broken humerus ; are each of them circumstances bearing no connexion yet known with fungus hæmatodes, but easily reconciled with carcinoma, (which can begin from injuries.) As to the fluid called *brown*, if it agrees with what I have seen in carcinoma, it is of a light snuff colour ; and can be of different degrees of *consistence.* I think also that I should have introduced the term *fatty*, in some part of the description, if the case agreed with what I have seen and found described by others in carcinoma : but perhaps it did not so agree.—The only case where *blood* appears to be particularly designated being at the *surface*, I find more and more reason to be confirmed in the opinion, that the fungus hæmatodes has less concern here, than carcinoma. As to the ramifications of blood vessels ; this is a memorable appearance in carcinoma ; where a superficial vein is sometimes so enlarged, as to have *appeared* to my eye of the size of a small horsewhip ; (when seen through the skin.)—I would ask, whether green elastic fat was seen in this arm ?

November, 1814.

REMARKS ON DR. HALE'S OPINIONS ON ANIMAL HEAT.

BY MR. B. C. BRODIE.

[From the London Medical and Physical Journal.]

In the last Number of your Journal, I see an account of an Inaugural Dissertation, by Dr. Hale, junior, in which the author states, that, having repeated some of my experiments on animal heat, he had met with very different results. This occasioned in me some surprise, as I had reason to believe that I had conducted my experiments with all that caution and attention to minute circumstances, which every physiologist knows to be absolutely necessary in such investigations, to enable us to arrive at any accurate conclusions. On perusing the article in question, however, I found that Dr. Hale in reality had not repeated my experiments, but had made others, bearing only a certain degree of analogy to them. The grounds of this assertion are, 1, that the whole of my comparative experiments were made on rabbits, and his appear to have been made on dogs ; 2, that in Dr. Hale's experiments the brain was left undisturbed, except by the division of the spinal marrow, whereas, in mine, either the brain was removed, or its functions were completely suspended by means of the Woorara poison ; 3, that Dr. Hale omitted to make the chemical examinations of the respired air.

1. In all my comparative experiments, I was led to employ rabbits in preference to other animals, because it was more easy to procure them exactly of the same age, size, and colour, (circumstances of great consequence to be attended to;) and because in my second series of experiments they were of a convenient size for being included in the apparatus constructed for the examination of the respired air. When in a rabbit the circulation is maintained by means of artificial respiration after decapitation, the animal lies motionless, and exhibiting no signs of sensibility; whereas a dog, under these circumstances, is affected with violent convulsions, and repeated evacuations of fæces, and even seems to perform certain voluntary movements. It is not unreasonable to suppose, that, if in a dog the other animal functions continue to be performed in so great a degree, from the influence of the spinal marrow, after the brain is removed, from the same cause the faculty of generating heat should continue to exist in a slight degree also.

2. Dr. Hale, in simply dividing the spinal marrow, left an extensive nervous communication between the brain and the trunk, by means of the nerves situated in the neck. In two of his experiments, indeed, he states, that these nerves were divided; by which I conclude, that he made a section of the sympathetic nerves, and of those of the eighth pair, in the middle of the neck, since no others are accessible to an operation, nor these except in this situation. Even this, however, cannot be regarded as having been sufficient to interrupt the nervous communication between the brain and the rest of the system, since the nerves of the face, tongue, &c. and the anastomosing branches of the sympathetic, spinal, and other nerves in the upper part of the neck, were still entire, and capable of conveying the cerebral influence directly to some parts, and indirectly to other parts of the body.

3. Dr. Hale, in neglecting to examine the respired air, omitted a very important part of the investigation. Had he found, as I did, that the animals in which the artificial breathing was employed, consumed as much oxygen, and evolved as much carbonic acid, as under ordinary circumstances, he would have regarded his experiments as equally conclusive with mine against the chemical theory of animal heat.

Indeed this part of physiology still presents an ample field of inquiry. On the subject of animal heat we possess at present some negative, but very little positive knowledge. The experiments of Dr. Hale are not without value; and I trust that he will continue to prosecute his researches. He will also, I am sure, excuse me if I take the liberty of suggesting, that, in the repetition of experiments made by others, too much attention cannot be paid to making them alike, even in the most minute particulars, as physiological science is not yet sufficiently advanced to enable us to pronounce with certainty what circumstances are, and what are not of importance.

B. C. BRODIE.

September 4, 1814.

PATHOLOGICAL AND PRACTICAL OBSERVATIONS.

[From the Edinburgh Medical and Surgical Journal.]

WHILE blood-letting is employed in such diseased conditions of the constitution to restore the balance of circulation, it is highly necessary to assist its operation, by promoting the natural evacuations also.* By so doing, we not only call forth the agency of the several excretories, in accomplishing the great work of purification, but we enable these organs to relieve themselves from accumulated or morbid secretions, which the constitutional disease so generally gives rise to. These morbid secretions are particularly to be met with in the course of the alimentary canal, where they impede the natural functions, produce local distress, and even exert a considerable influence, by means of some unexplained connexion, upon distant and sympathising parts.

It will be necessary to illustrate each of these points; and that I may not exhaust the patience of those who may accompany me through this long discussion, I shall endeavour to explain myself on this head as briefly as I can.

In attending to the effects of medicines which evacuate the stomach and bowels, we perceive some well-marked differences in their modes of operation.

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* The writer here alludes to a publication in a previous number of the same Journal.

Of emetics, some merely discharge the floating contents of the stomach; others cause it to throw off its mucous secretions. Purgatives exhibit a still greater variety, and are of three sorts; some evacuating the fecal contents of the intestines, others acting on their exhalent arteries, and producing copious watery stools; and a third class stimulating the mucous follicles, which so abundantly line the intestines, and causing them to expel the mucous matter they so copiously secrete. When the bowels are merely inactive, their secretions healthy, and no constitutional disease present, the simple aperients of the first class suffice to obviate costiveness, and prevent feculent accumulation.

The second are requisite when, in addition to unloading the intestines, it is desirable to abate arterial action, or allay fever, by reducing the quantity of the circulating fluid; and the third are required, either when the mucous secretions are so morbid as to give rise to disease, or where they are too copiously generated, in consequence of increased action in the vascular system. The particular medicines belonging to each class, will naturally suggest themselves to every practitioner; nor is it of importance to make any very accurate arrangement of them. The same medicines will act differently in different habits; and it is the effect on which attention must be fixed, rather than the means which accomplish it.

Two medicines only shall I particularize as deserving a more detailed discussion; and, in treating of them, I shall, if possible, contrive to embrace every topic connected with the present subject. The medicines I allude to, are antimony and calomel; the former an emetic, the latter a purgative, in which character only I mean here to consider it.

Each of these is remarkable for promoting mucous discharges, and, though belonging to different classes of evacuants, there is a strong analogy between their respective operations.

What antimony is to the stomach, calomel is to the intestines. But the resemblance is still closer, for calomel is capable of acting on the stomach, and of exciting vomiting, while antimony is well known to prove frequently purgative. Thus we see, that, in fact, the difference between them is in degree, not in kind; and that it consists only in calomel being more purgative, antimony more emetic.

According to the effect required, therefore, we have it in our power to supply, by one, that property in which the other is deficient; and, in the combinations of both, we possess a means of effectually evacuating the whole alimentary canal.

When it is considered that diseases of repletion are by far the most numerous that the human body is liable to; that the alimentary canal affords one of the most important outlets for discharging the redundancies of the system; that it is also a principal one for getting rid of the excrementitious impurities with which, in such diseases, the blood is speedily adulterated; and that the diseased secretions which accumulate within it are oftentimes a means of continuing, of complicating, and even of creating various diseases in different parts of the body, the value of such a remedy cannot fail to be duly appreciated. It remains for me to shew, that such morbid secretions do exist within the stomach and intestines, and that they do produce therein the effects now attributed to them, being the direct cause of some local complaints, while they beget also, by remote sympathies, diseases in distant parts. Such indeed is their influence in this respect, that they even re-act on the circulation, whose increased actions first gave rise to them, and cause it to continue appearances similar to those which result from a state of plethora, after this has been sufficiently relieved by evacuation.

Of the existence of superabundant mucus in the stomach and intestines, during inflammatory complaints, sufficient proof will be afforded, merely by inspecting the discharges brought off by particular evacuants, or occasionally by the natural efforts. With respect to the stomach, this examination may mislead, if only superficial; for the mucus being clear and colourless, is not readily distinguishable from the watery fluid surrounding it. Its tenacity, however, affords a ready means of demonstrating its presence; for, if a rod or wire of any kind is passed through the liquor and elevated, it will raise from the watery fluid the mucus existing therein, and sufficiently manifest its dense and visoid nature. It is this mucus that is produced by increased arterial action, affecting the mucous glands of the stomach in common with all other parts. To it, and to the action which produces it, as superseding the healthy actions of these parts, do I attribute the incipient nausea of fever, and of constitutional inflammation; and its expulsion I deem important, both as removing an injurious accumulation, and as enabling the secreting vessels, thus disencumbered, to continue those efforts, whose direct tendency is to relieve the general circulation, however inadequate they may be, when unassisted, to accomplish this purpose. Similar secretions are going forward also at such times, throughout the whole course of the intestinal canal; and are evidenced by the quantity of mucus which a dose of calomel or antimony, administered under these circumstances, uniformly expels. And to the power of these medicines, in dislodging such secretions, is owing much of the superior efficacy, so long and so justly attributed to them in the cure of diseases.

The want of sufficient attention being given to the peculiar effect produced by different purgatives, may perhaps suffice to account for the uncertainty and indecision which still prevail in their employment, notwithstanding the high authority which Dr. Hamilton's admirable work on this subject must carry with it. If this mucous matter is recently formed, and in no great abundance, a common purgative of the drastic kind will suffice to remove it, together with all such feculent lodgments as may have taken place in the intestines, A source of injurious irritation is thus removed; the various secreting and excreting vessels are left free to perform their natural functions; and the progress of nature, in her efforts to restore health, goes forward uninterruptedly.

If the mucous secretions are of older formation, and consequently more viscid, more tenacious, and more difficultly expelled, the common purgative fails to give relief, and a doubt is cast on the propriety of employing it, and on the veracity of former reports. The error here, however, is in employing a purgative inadequate to producing the effect required; for, if a competent dose of calomel and antimony is combined therewith to supply its deficiencies, the result will rarely occasion disappointment.

Further, if saline purgatives are given with the expectation of cleansing the intestines, when loaded with mucous secretions, they will very imperfectly effect this purpose; while, if persevered in, when the constitutional evacuations they procure are no longer necessary, they may wantonly exhaust strength, or otherwise injure.

Even the powerful remedy which a combination of calomel, antimony, and drastic purgatives affords us, may chance to be misapplied; and, if given when the bowels are irritable, with little mucus present, it is very possible that distress and injury may arise in this way, and a useful remedy fall into umerited disrepute. The occurrence, I believe, is rare, nor have I had occasion to witness any instance of such misapplication; but such might arise, and mislead a practitioner, not accustomed to close and accurate observation.

In fine, by neglecting to ascertain the nature of the evacuation required, and to apply, with accuracy, the remedy suited to procuring it, we must even run considerable risk of disappointment in the effects expected from purgative medicines, and may so misapply them, as really to do injury, even in cases where purging, judiciously regulated, is the best or only cure of the complaint.

The quantity of this mucus secreted in acute diseases is very considerable. It lines both the stomach and intestines, and causes many powerful medicines to pass through them without producing their ordinary effects; for, in consequence of the interposed mucus, the medicines come only imperfectly or not at all in contact with the living fibre, which alone they are capable of stimulating. It passes through, therefore, as if either the living fibre was torpid, or the medicine inert, when neither supposition is correct; and to mistake and accident we are occasionally indebted for illustrations of this subject, which perhaps regular practice would more slowly and imperfectly afford us. For the errors of dispensers, and the stupidity of patients, have not unfrequently afforded me instances of inordinate doses of purgative medicines being given with only moderate and salutary operation. A boy, eight years old, was attacked with scarlatina, with high inflammatory fever, and much redness of throat, threatening speedy ulceration. Purgatives were freely used, both drastic and saline. By mistake six powders were given, containing a drachm of rhubarb, ten grains of calomel, and six of antimonial powder, in eighteen hours, one at the end of every third, with the effect of only discharging much dark green slimy matter from the intestines, and of abating the general fever. Purgatives were in consequence thenceforward more freely employed, and the effect regarded more than the dose necessary for procuring it; and although the inflammatory fever run high, and was not allayed for many days, there did not occur a speck of ulceration on either tonsil. Neither did any of the ordinary sequelæ attend the disease, but the recovery was progressive and complete.

In the beginning of the present year, a female labouring under enteritis was bled and purged freely. Being a pauper, a dozen of purgative pills were ordered at once, on motives of economy. By mistake, a full dose was given along with a saline antimonial mixture, every four hours; and in sixteen hours she took a scruple of calomel, a scruple of extract of colocynth, and twelve grains of antimonial powder. Yet the effect was only salutary, and the stools merely such as it was every way desirable should be procured. We may hence infer the difficulty of establishing the precise doses of medicines to be administered, and must be conscious of the superior advantage of attending solely to the sensible operation, when this is capable of being ascertained, disregarding altogether the quantity of medicine necessary for effecting it. This is always possible with respect to purgative medicines, and to be accomplished by regular inspection of the alvine evacuations, without which the practitioner must remain in much doubt concerning some of the most important operations going forward within the body, and must labour under great disadvantages in accurately applying the remedies it is necessary to employ.

Disordered secretions, such as I have been noticing, are found to exist where no particular plethora or state of arterial action can be referred to as having given rise to them. These may either have remained after the subsidence of fever, or they may have arisen from local stimulation only,—a supposition by no means improbable, when it is considered how numerous and manifold are the articles of excitement which luxurious habits have introduced into our common sustenance. Be this as it may, such morbid secretions are very frequently found in the stomach, where they occasion a large proportion of gastric diseases.

I shall at present particularize one only, namely gastrodynia, which appears to me so clearly referable to this cause, and so successfully treated on this principle, that I have long ceased to entertain a doubt on the subject. In this disease opium is a remedy frequently resorted to, for the purpose of allaying the spasm, and relieving the pain; and, in obstinate cases, the quantity of laudanum occasionally required to allay a paroxysm is so enormous, as to induce, in the mind of the practitioner who employs it, a firm conviction, that the insensibility to ordinary doses is owing to the violence of the spasm, and that the excessive dose is actually necessary to preserve life. An old and experienced practitioner of this class once told me, with much self-complacency, that he had just given in full doses, quickly repeated, two hundred and sixty drops of laudanum, to relieve pain of stomach. My views of the disease, I own, are not in unison with such practice; nor shall I be inclined to alter them, so long as the means which I employ in treating the complaint continue so uniformly successful as I have hitherto found them.

Conceiving the pain to proceed from a contractile effort of the stomach to throw off from its surface the mucus which offends it, I have for many years laid aside the use of opium and stimulants, which merely repress the effect, without at all removing its cause, and which even tend to add to this, by stimulating the glands to increased secretion of the offending mucus, and have trusted solely to such medicines as act by expelling that matter to whose presence I attribute the complaint When pain is severe, it may no doubt be advisable to allay this by a dose of opium, or some other stimulant. Nor is any injury sustained thereby, provided the real disease is borne in mind, and properly treated. But unless the severity of pain is great, I own I should be averse to relieving it in this way, or by any means but a removal of the offending matter,—as the relief to pain consequent upon such evacuation may be relied on as announcing the radical cure of the complaint. In some hundred cases that I have now treated on these principles, and which, of course, have presented almost every variety of kind and degree, including the form of pyrosis, which Cullen has pronounced to be so obstinate, I have in no instance given a single grain of opium, or failed of giving decided relief. Almost the only medicine I employ is a purgative, composed of extract of colocynth, calomel, and antimonial powder, and a moderate dose, given twice in the week, seldom fails to cure the disease, ere half a dozen are thus taken. Adjuvants I seldom employ, but they may no doubt be occasionally serviceable,—magnesia, and alkalies, especially ammonia, will correct acidity, and relieve heartburn, and other dyspeptic symptoms may derive benefit from other treatment; but when the foregoing purgative has been a few times employed, I very generally find, that all dyspeptic symptoms disappear, and leave the patient free from complaint.

The disease of colic I believe to be precisely analogous to gastrodynia, both in its pathology and treatment; and to differ only in being more prone to pass into inflammation.

The remote sympathies which different parts of the body evince, under disordered conditions of the stomach and digestive organs, have often engaged the attention of practitioners, and been recorded by practical writers. In no one, however, is the profession so much indebted for illustrating this subject as to Mr. Abernethy, whose treatise "On the Constitutional Origin and Treatment of Local Diseases," affords an unquestionable proof of his distinguished talents, and must ever be deemed a most valuable acquisition to medical science. My present intention is only to notice a few of such morbid sympathies as have occurred in the course of my own experience, and I do so, still with a reference to my fundamental purpose, and in order to establish the principles by which blood-letting should be regulated and assisted in the cure of diseases, by exposing some sources of deception, which, if not understood, might thwart its beneficial effects, and weaken the confidence that deserves to be placed in it.

I have mentioned, that in all complaints attended with fever or constitutional inflammation, the gastric and intestinal secretions are quickly increased. I have stated also, that accumulations of morbid secretions oftentimes take place in the alimentary canal, of slow and gradual formation, and not referable, by any distinct or well-marked connexion, to a state of generally increased arterial action, The former state may even be superinduced upon the latter; and thus an additional complication, both of diseased action and of diseased conditions, ensue.

A disease in which this morbid state of the secretion exerts considerable influence, is rheumatism; and to the neglect of attending sufficiently to this, may be attributed much of the contradictory opinions which even practical writers have advanced respecting this complaint, and of the apparently incongruous treatment they have recommended.

Some consider all rheumatic affections as inflammatory, and to be treated by evacuations. Others assert, that evacuations may be pursued to the injury of the constitution, and yet the rheumatism be not removed. These, unwilling to employ so active a remedy as blood-letting, in a case that may resist its efficacy, are inclined to trust rather to diaphoretics, opiates, and tonics, as a safer alternative, however tedious or imperfect the recovery which they obtain.

Now, that acute rheumatism is a highly inflammatory complaint, can hardly admit of a question ; the degree of inflammatory fever, and of strong arterial action that attends it, place this beyond a doubt. Though blood-letting, however, is the only remedy for speedily allaying the inordinate action of the arteries, and for arresting the progress of disorganization in the affected joints, yet it is true, that the pains do not always abate in proportion as fever is allayed, but continue to tease the patient, under the form of chronic rheumatism, long after the disease has ceased to be characterized by increased vascular action. In several such cases I have had evidence the most direct for assigning this continuance of disease to the influence of morbid secretions within the stomach and intestines, and by the full effect of an antimonial emetic, have succeeded in dislodging from the stomach a load of dense, viscid, and opake mucus, followed by copious stools, dark, slimy, and fetid,—and this even when the most active purgatives had been assiduously employed for weeks preceding. The effect on the disease has been an immediate alleviation of pain, a restoration of sound and refreshing sleep, which the nocturnal paroxysms had long prevented, and a cessation of the copious perspirations, which had for many previous nights drenched the patient, without in the least alleviating his sufferings ; and this latter circumstance, I think, tends to throw much light on the history of rheumatism, and to show that it is not by diaphoresis that remedies of this class so often afford relief, but by some operation connected therewith, and to whose efficacy this contingent operation is not even essential.

I shall now briefly notice a case or two, in order to confirm the observations I have just been making.

A female, thirty-two years of age, had for eleven years been a cripple from contractions of the joints, occasioned by severe rheumatism. For several years afterwards she was subject to habitual diarrhœa, and, during its continuance, was free from her rheumatic complaints. The diarrhœa ceased, the rheumatism returned, and, for nine weeks before I saw her, she had been in constant pain, and almost without rest.

She had pains of ancles, knees, hip-joints, and loins, a furred tongue, costive bowels, and pulse at one hundred and thirty. The heat of skin was temperate, and she was covered with a copious eruption of urticaria, which had appeared a few days preceding. By purgatives she was in a few days considerably relieved, but, though the other symptoms were gradually abating, the pulse could not be got under one hundred and twenty-six. Believing the cause of disease to be now more peculiarly in the stomach, I ordered an antimonial emetic. It operated with some severity, owing, as I conceive, to the tenacity with which the viscid mucus adhered to the stomach. She succeeded however in discharging an immense load of dense, viscid, opake phlegm, and nearly at the same time passed several copious stools, most foul and fetid, although for a week she had been freely purged, almost every day, by means of calomel and extract of colocynth.

The stools I did not see, as their intolerable fetor required them to be speedily removed.

The effect of evacuating them, however, was almost instantaneous; for on that night she slept profoundly, and next morning was free from all pain, with a pulse full, soft, regular, and at ninety-two. By correspondent treatment, successive returns of disease were afterwards speedily and effectually relieved; her health in a short time become materially improved; and even the diseased and crippled joints were eventually so much amended, as greatly to increase her powers both of locomotion and of personal exertion of every kind.

The next case which I have recorded, was of a corpulent plethoric man of about forty. He was attacked by acute lumbago, which soon yielded to blood-letting and purging. Incautious in the management of himself, he soon returned to me with severe rheumatism of the chronic kind, under which he had suffered for ten days. The pains affected successively every joint; they deprived him of all rest; and both his arms and legs were nearly powerless, insomuch as to render him extremely low-spirited, and apprehensive of losing the use of them altogether. His tongue was moist, and slightly furred; his skin temperate; and pulse soft, and under ninety. His bowels had been freely purged; for the only relief he had obtained, while trusting to his own management, was from the use of a purgative mixture I had previously ordered for him. Having been copiously purged the day on which I saw him, I ordered him only a dose of calomel, antimonial powder and opium, to be taken at bed-time. No relief, however, was obtained.

Next day I inquired respecting the state of his stomach, which he assured me was perfectly well, and free from nausea or any uneasy sensation. I ordered him, notwithstanding, an antimonial emetic, which he reluctantly agreed to take, being much disposed to question its propriety. With its effects, however, he was afterwards perfectly satisfied; for, after its operation, he walked up stairs, with the perfect use of all his limbs,—an exertion which he was previously utterly incapable of; slept soundly; had no perspiration; and arose next morning without one uneasy feeling, save a sense of weakness which he experienced in the limbs that had been principally affected.

The matter rejected by vomiting was not transparent mucus; but, as in the former case, consisted of a dense, viscid, opake phlegm, in large quantity. Several foul and fetid stools, too, were parted with, both during and immediately after the vomiting. The appearance of the matter rejected by the stomach, was such as no one could suppose to be a recent or a healthy secretion; and the fact of so morbid a state of stomach existing, without its being evinced by any of the ordinary indications, either of uneasy sensation or of depraved appetite, is well worth attending to.

It is also well worthy of remark, that such copious fetid stools so immediately ensued upon the action of vomiting, as almost to preclude the idea of their being occasioned by a direct effect of the antimony on the bowels; and to induce a belief, that they were owing to a simultaneous effort of the intestines sympathetically taking place, at the moment the stomach was parting with its diseased secretions.

The connexion between these several diseased secretions, and the rheumatic affection, in both the cases I have now reported, cannot, I conceive, well be doubted; and the well-marked effects of the evacuations should lead us, I think, to a more accurate conception of the mode in which alterative doses of antimony so frequently prove serviceable, in curing this disease.

In these cases relief might, and no doubt would have ensued on such alterative doses, persevered in for a sufficient length of time; but then the *modus operandi* must have remained involved in much obscurity, which, by the speedier operation, and more obvious effects of the emetic, seems happily dissipated.

The cases themselves, to which I could, if necessary, add several others I deem valuable, more from the light which they shed on the general pathology of the body, and especially on that of rheumatism, than from the specific practice employed.

Indeed, I should be even inclined to hesitate in recommending its general adoption, and am much disposed to doubt the propriety of resorting to such full vomiting, unless where milder means are found ineffectual.

In consequence of the stomach being so firmly coated with its viscid lining, large doses of tartarized antimony are required to excite full vomiting. From twelve to sixteen grains are frequently necessary, though given in doses of from a grain to a grain and a half every ten minutes; and the operation, when at length excited, is often extremely severe. Notwithstanding the frequent evidence which I have had, therefore, of the speedy and complete effect of full vomiting in such complaints, I have not of late been in the habit of employing it, in consequence of the distress it occasions, but have trusted rather to small doses of tartarized antimony given at some hours interval, and so increased, as at length to excite either vomiting or purging. The operation of vomiting, when thus induced, is always mild; the stomach gradually throwing off its mucous lining, which, when once lodged in its cavity, a gentle effort suffices to expel, without either severe straining, or continued sickness ensuing.

Whether in all cases this gentle operation would be equally effectual as full vomiting, in dislodging such morbid matter as was discharged in the two foregoing cases, I am not prepared to pronounce. Whether there are cases, wherein the fuller operation, notwithstanding all the suffering that attends it, may not be preferable, in order to give more speedy relief to contracted or paralysed limbs, or to arrest the progress of disorganization, is what time and experience only can determine. It must occur to all who read these remarks, that many which are here applied to rheumatism are equally applicable to the kindred disease of gout, and that, connecting them with what has been so recently advanced respecting the state of plethora in which this disease originates, they tend still further to illustrate its pathology, and to point out the practice necessary for its relief They assist, too, in explaining much of that extraordinary success, which has occasionally attended the exhibition of the celebrated *eau medicinale;* for, if the local inflammation which occurs in gout and rheumatism, is so intimately connected with the presence of morbid secretion in the alimentary canal, it is not surprising that so powerful an evacuant as the *eau medicinale* should remove the local complaint as speedily as the antimonial emetic did the rheumatic affections in the foregoing cases. Admitting, then, the pathology to be correct, which attributes gout to the existence of a state of plethora and inflammatory action in the blood-vessels, and the influence of vitiated secretions within the alimentary canal, (which latter may be regarded in a great degree as the natural product of the former,) does it not seem to be fully within our power to bring this hitherto inveterate disease under the control of rational practice? And may we not hope to treat it as effectually, and much more safely, by the well-ascertained powers of such a remedy as a combination of colocynth, calomel, and antimony presents us with, as by the less manageable means of white hellebore, or the precarious and uncertain *eau medicinale?*

And this valuable form of purgative medicine I have frequently had occasion to employ with gouty patients, whose previous attacks had been left to nature, and who have been agreeably surprised to find, that, after the evacuations procured by it, the gouty attack subsided without those reiterations they had formerly experienced. That a remedy is thus discovered for gout, I by no means hold forth; for it is utterly inconsistent with my views of the disease to conceive that any such can ever have existence. Believing, as I do, that it is caused, not by any specific virus, or morbid poison, capable of being neutralized by an antidote, but by a series of natural processes originating in a state of plethora, I can form no conception of any remedy admitting of indiscriminate application, or that can be suited to all the varied states in which this disease is presented to us. The means I would recommend are advocated, not from their possessing any secret or unexplained powers over the disease, but from their being pointed out by a rational pathology, and fully established, both with respect to their safety and efficacy, by extensive experience. For there is no period of medical history, which does not afford us ample evidence of such treatment having been successfully pursued; although prevailing prejudices, and the strong tendency which gouty patients so generally evince to yield to any delusion which affects to relieve their complaints without debarring them from the excesses which produced these, have ever given preponderance to the opposite system. To those who are sceptical on this point, I would recommend the perusal of Mr. Ring's late excellent Treatise on Gout, which reviews the treatment of the disease from the earliest writers to the present day, and satisfactorily exhibits a host of authorities, fully sufficient to uphold any practitioner in the employment of evacuations, whenever his own judgment shall direct him to prescribe them.

No apology, I trust, is necessary for the minuteness with which this latter part of my subject has been treated, nor for the digressions into which the wish of fully illustrating it, may have led me.

E. BARLOW, M. D.

Bath.

*Analysis of the preceding Essay.**

Prefatory remarks. Outline of the animal economy.

Process of Digestion.

Deficient nutrition—from want of food—inability to receive it—impaired digestion.

Excessive Nutrition.

Pulmonary circulation; its effect in perfecting the blood formed by the chyle. Aortal circulation, and the distribution of blood for the purposes of life. Absorption of effete matter, and its return into the circulation. Excretion by the skin, intestines, kidneys, liver.

The foregoing animal processes, reconsidered with a view to their classification. Divided into the five following:

First Class.—Process of nutrition, as performed by the digestive organs.

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* This essay includes a series of papers published previously to the one inserted here.

Second Class.—Process of sanguification as perfected by the pulmonary circulation.

Third Class.—Processes of secretion, as performed in the course of the aortal circulation, by the several secretory organs, both nutrient and glandular.

Fourth Class.—Absorption, as performed by the lymphatics.

Fifth Class.—Excretion, as accomplished by the several emunctories.

Illustration of the First Class.

Effects of repletion. Natural efforts excited thereby, and their tendency to remove it. When inadequate, disease induced.

Illustrations of the Second Class.

Action of the lungs excited by a twofold stimulus, namely, of the air, and of the blood; the latter deemed the principal one in inducing disease.

Illustrations of the Third Class.

Arteries excited by arterial blood; also by diffusible stimulants received into the stomach; the effects of which, however, in producing disease, are in a great measure dependant on the degree of repletion existing.

Illustrations of the Fourth Class.

Absorbents excited at their extremities by the effete matter of the system, and, throughout their course, by their lymphatic contents. The power of direct or specific stimulants on them questionable.

Illustrations of the Fifth Class.

Excretory organs stimulated by suitable blood. Their actions promoted also by exercise.

Analysis of the Second Part.

Introductory remarks. Apology for considering nervous influence as a secretion, and for referring it to the third class of animal processes.

Pathological principles, as deduced from the foregoing physiology, and from the phenomena of diseases.

Incipient stages of disease introductory to a state of fever or inflammation.

Symptoms whereby they are characterised.

Transition into fever or inflammation.

Effects of depletion in relieving the incipient stages, with some rules for regulating the employment of blood-letting.

Auxiliary means to be employed in aid of blood-letting.

Sympathetic influence of morbid secretions found in the alimentary canal.

REMARKS ON THE NATURE AND TREATMENT OF PURIFORM OPHTHALMIA.

BY WILLIAM JAMES WILSON, *Fellow of the Royal College of Surgeons, London, and Surgeon in Manchester.*

Few diseases, to which the organ of sight is liable, have a more direct tendency to effect its destruction, than that species of ophthalmia, which, from a remarkable symptom attendant on it, has been denominated puriform. Much praise is due to the late Mr. Saunders, for a very accurate history of the disease; indeed, before his essay on the subject appeared, practitioners were not well acquainted with the nature of certain organic changes which take place when the complaint assumes a violent form. These changes are the effects of excessive action in the vessels of the conjunctiva, eventually extending to that part of the membrane which covers the front of the transparent cornea. In common ophthalmia, the disease is often confined to the conjunctiva of the palpebræ and sclerotica; and, even where it extends to the cornea, the inflammation, for the most part, terminates in the adhesive stage, and more or less opacity is the consequence. But, in cases of that form now under consideration, there is but little disposition to the effusion of coagulable lymph. It, however, sometimes takes place; and, provided the inflammation has begun to decline, may be looked upon as a favourable symptom. In extreme cases, the conjunctiva sloughs; and as that part of it which passes over, and is most firmly attached to the cornea, has but little power to withstand disease, a slough, or sloughing ulcer, first forms on the conjunctiva of the cornea. Eventually, the proper lamellæ of the cornea partake of the same morbid actions; but I believe it is only from contiguity that the actual substance of this tunic becomes affected, the conjunctiva being the primary seat of disease. In proof of this, we may observe, that the slough first formed is superficial, and soon cast off. Afterwards, if the disease is not checked, the sloughs are deeper, and more time is required for their separation. In no instance have I observed the anterior chamber to be opened at once; but where this unpleasant circumstance has happened, it has been by a *succession* of sloughs, or the spreading of ill-conditioned ulceration. The puriform ophthalmia of the infant, and that of the adult, often termed the Egyptian ophthalmia, produce the same effects on the conjunctiva and cornea. When the inflammatory symptoms run very high, sloughing is the consequence; when less violent in degree, an effusion of coagulable lymph may take place. The opacity produced by this effusion is to be carefully distinguished from a peculiar dusky appearance observable on the cornea, when a portion of it is about to slough; an appearance much to be dreaded. It is, indeed, as a late writer has well expressed, the "unerring harbinger of mortification." When the disease has been neglected, or we have not been able to put a stop to the sloughing process, the anterior chamber of the aqueous humour becomes opened. The iris then protrudes, and, in its turn, ulcerates; finally, the lens and vitreous humour issue through the breach in the cornea. This concludes the sad eventful history of a case of severe puriform ophthalmia. It fortunately happens, that such a termination is rare, compared with the frequency of the complaint. Even should the anterior chamber become opened, and the iris protrude, provided the aperture in the cornea is but small, and we pursue the proper treatment, the iris will adhere to the edges of the ulcer; lymph will be effused, and the healing process soon be completed. Though the iris will be forever adherent to the cornea at that part, yet vision will not be materially impaired. It must be understood, however, that we can only hope for this favourable termination, when the circumstances I have mentioned occur in a slight degree; for, not to speak of the extensive opacity of the cornea, left on the healing of a large ulcer, the pupil will become so deranged, from being dragged under the opaque portion of the membrane to which it is adherent, as to prevent the passage of rays through it; nay, sometimes the pupillary edge of the iris will become fixed throughout its circumference, to a central penetrating ulcer of the cornea. Here it is obvious, that nothing will be likely to benefit the patient, but his submitting to an operation for cutting the iris, and removing the crystalline body, which in these cases I have always found opake. But this is a subject foreign to my present purpose. It is not my intention at this time to enter into a minute detail of the various measures to be adopted in the treatment of puriform ophthalmia. Indeed I wish to confine my observations to a consideration of the remedies to be used, where sloughs, or sloughing ulcers, have formed on the cornea. From the rapidity with which the disease runs through its course, those afflicted will frequently not apply for relief until the violence of the inflammation has begun to decline. It is about this period that the structure of the cornea suffers.

In every case of puriform ophthalmia, it is the duty of a surgeon to make himself acquainted with the actual state of the organ; and this object is frequently not very easy to accomplish, owing to the great tumefaction of the eye-lids. On an attempt being made to separate the palpebræ in the common way, the orbicularis muscle contracts, and projects the tumid conjunctiva, obscuring the whole ball of the eye. From having been witness to perhaps some of the worst cases that can possibly happen, I believe I am warranted in saying, that in no instance need we fail in our attempts to obtain a view of the cornea.

It is rather difficult at first to get the method of separating the eye-lids, so as to prevent the distended conjunctiva from protruding. When difficulty occurs, I always use Pellier's speculum, which is, I think preferable to any other instrument. On this subject I can speak with a good deal of confidence, having, whilst a pupil of the London Eye Infirmary had daily opportunities of inspecting purulent eyes. I generally succeed best in the following manner: I first wipe the eye-lids from any discharge or moisture which may happen to be on them; and placing the fore-finger of my right hand exactly on the edge of the tarsus of the upper lid, and my left thumb on the tarsus of the lower, I do not begin immediately to separate the palpebræ, but endeavour to get the swollen conjunctiva quite under the lids. Having done this, I press the tarsi upwards and downwards, fixing them completely on the bony margins of the orbit. By this means, the conjunctiva of the palpebræ is kept completely under the cartilaginous edges of the lids.

This is the way in which I make an examination with the fingers; but no general rule can well be given, as I imagine different surgeons may have different methods, equally good. Where chemosis is present, in any remarkable degree, the speculum is, perhaps, preferable to the fingers, as the discharge is apt to get under the latter, and cause them to slip. Notwithstanding all Mr. Saunders has said relative to an inspection of the cornea, I am convinced that it is too much the custom to decline examination when any difficulty occurs. The complaints of a young patient, and the ill-timed interference of his friends, will also do much to deter a young practitioner. But when we consider how important it is, both with regard to our prognosis and treatment, no one will surely deny, that a knowledge of the precise state of the cornea is indispensably necessary. We may, indeed, sometimes *guess* that sloughs are forming on the cornea; for, in some particular habits, the general powers of the constitution become much depressed, the pulse is languid, and there is a tendency to profuse perspiration. We are not to wait for these symptoms before we examine, or to flatter ourselves, where they are absent, that all is going on well. I have selected the following case, from a vast number, as I think it shews several interesting facts in a very striking light:

CASE.

April 2d, 1814.—I was desired to visit George Jones, a weakly, delicate boy, five years of age, and of a scrophulous habit. The disease had existed several days when I first saw him. I examined his eyes. The palpebræ were enormously swollen; puriform discharge very profuse. On the external margin of the cornea of the right eye, was a dusky elevated opake spot, denoting that a slough had formed. The whole cornea was cloudy, but this latter appearance was totally different from that exhibited at the particular part I have mentioned. The conjunctiva of the left eye was most acutely inflamed; diffused opacity of the cornea, but no appearance of slough or ulcer. I injected a weak solution of alum on both eyes, and directed that it should be repeated every four hours; and, in the intervals, to keep the eye-lids moist with common saturnine wash. Four leeches were applied to the left eye.

R. Ext. cinchonæ, gr. x. in pil. ij. dividend. 4tis horis sumend.

3d.—The sloughy spot of the right cornea was elevated, and had a groove around it; palpebræ less swollen. The bark was continued.

4th.—His pulse, which had hitherto been small and languid, had now acquired a good deal of firmness and regularity; puriform discharge lessened; palpebræ much less swollen. On injecting the right eye, I washed away the slough, which came off in one piece. The ulcer left by this separation has ragged edges, with a transparent bottom; the left cornea so opake, the iris cannot be seen through it The alum injection was omitted, as well as the bark. A purgative powder was given, and two leeches applied to the left eye-lid.

5th.—The ulcer of the right cornea is extending; the discharge is of a light yellow colour, and more viscid; left cornea a good deal clearer. I injected a solution of alum on the ulcer of the right cornea, and again gave the bark in the same dose as before.

7th.—Lymph is deposited in the bottom and on the margin of the ulcer; the inflammation in the left eye has increased; the cornea is again so opake that the iris cannot be seen through it; discharge very profuse. A dose of calomel and jalap was ordered; four leeches applied to the left palpebræ; bark omitted.

8th.—Ulcer of the right cornea healing; left cornea rather clearer; tumefaction of the eye-lids and discharge abating.

10*th.*—Ulcer of cornea nearly healed; left cornea nearly clear, except a small central speck; bowels confined. Repeat the purge, and use a common saturnine wash.

12*th.*—Tumefaction so much subsided that he opens both eyes; his vision is tolerably good in the left eye; he marked out to me the figures on the face of a watch; there is a large opake spot on the outer margin of the right cornea, left on the healing of the ulcer; discharge has not quite ceased. A weak solution of sulphate of zinc was ordered as a lotion. Acid. sulph. dil. gr. vi. ter de die.

18*th*.—The active inflammation has ceased in both eyes, but there is a profuse discharge of tears; they are morbidly sensible to the light.

22*d*.—I directed that a small quantity of a mixture, consisting of equal parts of vin. opii and water, should be dropped on the left cornea daily.

24*th*.—His general health is much improved; opacity of the left cornea diminished. Continue the acid twice a day.

My patient went on well till the 28th, when I was again desired to see him. I found that he had, the day before, been attacked by inflammation in both eyes; the right conjunctiva was very vascular, and discharged a puriform fluid; the left eye was but slightly inflamed; the palpebræ were beginning to swell. Three leeches were applied to each eye; a purgative was given; and I injected a solution of alum.

29*th*.—Right eye-lids enormously swollen; puriform discharge increased; right cornea quite opake; no slough or ulcer; left eye but slightly inflamed; complains of great pain in his head; pulse quick, with hot and dry skin. Two leeches to the right eye; a table-spoonful of common saline mixture, with four drops tinct. digitalis every three hours. I injected the solution of alum.

30*th*.—The centre of the right cornea has become elevated; pain in the head very violent; stools offensive and dark-coloured; breath foul. A powder of calomel and rhubarb was ordered.

May 1*st*.—Central opake spot of the cornea is evidently a slough; at its upper part it is beginning to separate; there is also a dusky coloured spot on the cicatrix of the old ulcer; still complains of pain in his head; inflammation of the left eye ceased. I omitted the alum injection.

2*d*.—Nearly one half of the cornea has sloughed and separated; the bottom of the ulcer is also in a sloughy state; no headach; pulse feeble. Ext. cinchon, gr. x. 4tis horis. I injected a solution of alum on the ulcer.

3*d*.—Ulceration extending superficially along the cornea; the bottom of the ulcer is of a dirty yellow colour; he is much emaciated, and so weak he can scarcely stand.

5*th*.—The deep-seated slough at the bottom of the ulcer is separating.

7th.—The slough has separated, and the anterior chamber is opened; the iris is in contact with the edges of the ulcer of the cornea. Has continued his bark since the 2d; appetite improved, and he gains strength.

8th.—The iris protrudes; ulcer of the cornea very irritable. I injected a weak solution of nitrate of silver, and added acid. sulph. dil. gr. v. to be taken in a little water, with each dose of bark.

9th.—The portion of protruded iris has ulcerated; something came through the aperture in the cornea, which I took to be the lens in an opake state; the size of the eye-ball is much diminished; tumefaction of the palpebræ and discharge have quite ceased.

11th.—The parts are now taking on healthy actions; lymph is effused from the remaining part of the cornea.

12th.—Red vessels are shooting into the lymph.

15th.—The ulcer is closing fast. Since the 3d his bowels have been occasionally emptied by some mild aperient; the bark was omitted.

17th.—Ulcer nearly healed, the cornea is only transparent at its inner margin; has lost all useful vision in that eye; left eye continues well.

22d.—Ulcer is quite healed, and the strength of my patient being somewhat recruited, he left Manchester to enjoy the benefit of sea air.

Vehement local action often occurs, where the actual powers of the part and general constitution are but weak. In no instance is this more plainly shewn, than in some cases of sloughing of the cornea. The death of a portion of the cornea, in a young and tolerably healthy subject, takes place from violent inflammation, as may happen in any other part of the body. Here, generally, when the slough is cast out, the restorative process will go on uninterruptedly; but, in the scrofulous or otherwise debilitated habit, it is widely different. The cornea is a part but feebly endowed with living power; a great effort of the constitution is therefore required to enable it to repair its diseases; so we find, that should the slough of the cornea, occurring in certain habits, become separated, the contiguous parts, participating in the morbid actions, also die. The healing of an ulcer of the cornea differs not from that of any other ulcer. Lymph must be effused;

that lymph must be organized; the process of granulation must be completed. Do we not every day see this circumstance happen? An ulcer (let us say on the leg) is healing, granulations have shot forth, but the constitution, from some cause or other, becomes disturbed; suddenly the new formed parts are absorbed, and a deep foul ulcer is left. So again it is with the ulcer of the cornea. Where the constitution has begun to flag, I have seen an ulcer of the cornea, just on the point of healing, have its granulations all absorbed, or large portions of the membrane die, till the whole has been destroyed. In the case I have related, it will be observed that I gave bark in pretty large doses. I have, however, I hope, clearly defined the circumstances under which it was administered. The pulse was feeble; the sloughy spot on the cornea exhibited a well known appearance. I was convinced that, when this slough separated, there would be little power to heal the ulcer. A spreading of this ulcer must therefore in all probability happen. At my third visit, finding my patient's general state so much improved, I omitted the bark, being unwilling to excite, even indirectly, too vigorous an action in parts already enfeebled by disease. I certainly expected that I should soon see the ulcer put on marks of healing. In this I was disappointed; for on the 5th the ulcer had an unpleasant appearance, and was extending rapidly. The bark was again given till the 7th, when, lymph being deposited round the margin, and at the bottom of the ulcer, it was a second time omitted. At the period when the tonic plan is to be adopted, the bowels ought to be regulated with the mildest laxatives, but purging is to be avoided. In the scrofulous subject, after the tumefaction and purulent discharge have ceased, and the ulcers healed, if there have been any, the complaint frequently assumes a chronic form; there is a profuse secretion of tears, and the eyes are morbidly sensible to the light; in short, a species of scrofulous ophthalmia succeeds the puriform. In these cases, I have seen the most decided benefit arise from the exhibition of small doses of calomel and opium combined. The constitution should also be invigorated by tonics, sea-air, &c. We must here be careful not to apply local stimulants too soon, even though there is no appearance of general increased vascular action in the organ. Here I have seen much mischief arise, from the indiscriminate use of the *vinum opii;* though there are cases requiring it. I am much inclined to attribute the second attack of inflammation in Jones's case, to the inadvertent application of a handkerchief to his eyes, which had been stained with the former purulent discharge; but I could never exactly ascertain the fact. The different stages of the disease were admirably shewn: 1st, On the left eye, the termination in the adhesive stage leaving opacity. 2dly, The striking difference in the appearances exhibited by this opacity, and the opake spot on the right cornea, previous to the formation of a slough. 3dly, The efficacy of bark in putting a stop to the sloughing; and though in the second attack it failed to do so, until the organ was almost destroyed, yet in the end it did accomplish the object for which it was administered. Had my principal view in relating the case, been to confirm the fact that cinchona is the remedy on which we must often place our chief reliance in sloughing of the cornea, I could have brought a host of instances. Lastly, The process set up by nature to repair the disease, even when the whole substance of the cornea has been destroyed. Here there is an attempt to form a new cornea, but it is permanently opake.

From the Edinburgh Medical and Surgical Journal.

MANCHESTER, June 4th, 1814.

Memoir upon the Compound and Smooth or Simple Eyes of Insects, and on the Manner in which these two Species of Eyes concur in Vision.

BY M. MARCEL DE SERRES, Professor of the Sciences in the Imperial University.*

> Visus igitur quo insecta gaudent nulla penitus ratione cum nostris oculis aut cum camera obscura in qua rerum species, reflexionis ope, super charta aut panno albicante pinguuntur in comparationem venire potest.
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> SWAMMERDAM, *Biblia Naturæ,* tom. i, p. 502.

SIGHT seems to be one of the most perfect of the senses of insects; and in this respect these animals are in the first line among those without vertebræ, as birds are among those with vertebræ. Both enjoy a very long sight; and we know at how great a distance the maggot, like the bird of prey, perceives the object which it wishes to devour. Is the great quantity of air inhaled by these two orders of animals in the act of respiration, the cause of the acuteness of this sense, as it is of their muscular strength? or must we account for it from the great development of their retina and the largeness of their eyes? However difficult it may be to resolve a question abounding so much with difficulty, it is probable that insects and birds owe equally to the great abundance of air which they inhale, the acuteness of their senses, and the activity of their digestion; finally, the violence of their passions, if we may so express ourselves.

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* *Phil. Mag.* from *Magasin Encyclopedique,* February, 1814.

Far different from the vertebral animals in the structure of their eyes, insects enjoy the impression of external objects by means peculiar to themselves, and which furnish proofs that nature knows how to attain the same ends by very opposite ways. In fact, the operation of vision is not performed by insects in the same way as with most animals by the action of the luminous rays, which, after passing through the pupil, collect on the retina, but rather by the shaking of the optic nerves occasioned by the light which passes through the cornea. These eyes are constructed so as to receive the images of objects by the simple shock of the rays which these objects reflect, and this method of feeling must necessarily be very acute. Besides, insects not having, like the vertebral animals, pupils susceptible of contracting, it would seem that vision ought to be very perfect with them, on account of the great number of rays which fall continually on the facets of their eyes.

Nevertheless, one of the great inconveniences which results from the organization of insects, is the kind of immoveability of the parts in which their eyes are fixed. But nature has remedied whatever is unfavourable in this organization, by rendering the eyes of insects very much complicated, and by multiplying their facets in such a way, that one and the same eye presents as many as fourteen thousand, as Hooke observed in the *libellulæ*.* Nature has even sometimes multiplied the number of the eyes themselves, and this number seems to be always in the ratio of the size of these very eyes, and the immoveability of the parts in which they are situated. Thus, when the compound eyes are entirely wanting, the number of the flat or simple eyes is greatly increased, and certain species have thus even six pair of eyes. This is observed in some of the apteræ, many of the larvæ, and particularly in the spider. Indeed, the latter should be separated from the insects as Messrs. Lamarck and Cuvier have already done; for their organization presents some very marked distinctions, and of the first importance. In fact, the spider, like the scorpion, has a kind of lungs, and consequently a real heart with blood-vessels; whereas, in the real insect, the organs of respiration are always ramified, and the vascular system, or at least what has been taken for it, never is. Sometimes, however, the eyes of insects are not situated in parts entirely immoveable, and certain species present a kind of neck, which Comparetti* seems to have been the first to observe, and which permits the head to execute a variety of movements. This neck is very conspicuous in some of the carnivorous species, which not being able to pursue their prey on account of the disproportion of their organs of movement, require the faculty of seeing to a great distance. This organization is very evident in the *mantis*, the *empusa*, and the *mantispa*, in which the head may be directed to one side, or backward, and may even be twisted almost round. It may be remarked, in general, that the eyes are the more convex and the more salient, the more decidedly carnivorous the insect is, as the mobility of the parts in which they are situated, is always relative to the kind of life and to the habitudes of the insect. In short, we never saw the eyes of these animals present any kind of movement, and they always adhere to the parts where they are situated, and consequently are completely immoveable.

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* Swammerdam, *Biblia Naturæ*, tom. i, p. 490, and *Collection Academique*, p. 323. Hooke has given the eyes of the *Libellulæ* in his *Micographia*, plates xxiii and xxiv.

The sight is exercised by insects by two organs very different from those which perform this function in the vertebral animals and the *mollusci:* they exhibit even very striking differences from each other, as well in respect of their external conformation, as in their internal disposition. Some appear to be a large cluster of eyes united together, if we may so express ourselves, and others exhibit one only. The former have been called *compound eyes*, on account of their arrangement; and the latter smooth or simple eyes, on account of their simplicity. These two kinds of eyes concur to the same object, and are sometimes united on the same individual: thus the *orthopteræ*, like the *hymenopteræ*, present at once both compound and simple eyes, a disposition which is also observed in various kinds of *hemipteræ*, *neuropteræ*, and *dipteræ*. The other classes present only a single kind of eye; but, when there are only smooth eyes, their number is always considerable, probably in order to make up for their imperfection. In general, all winged insects have compound eyes, and this law does not seem to present any exception. In the *apteræ*, certain kinds present only compound ideas, such as the *cloportæ*: in truth, the latter seem formed by a collection of simple eyes. Other kinds have only simple eyes; such are the *julæ* and the *scolopendræ*, which present a certain number of simple eyes; but all these animals appear to me so different from the insect tribe in point of organization, that I have thought it right to pass them over.

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* *Dinamica Animale*, part i, p. 103 to 106.

With respect to the larvæ, those of half-metamorphosed insects uniformly have eyes similar to those of the perfect insects; whereas the larvæ of insects wholly metamorphosed have only simple eyes, which vary much in their number and position. The caterpillar has six or eight on the sides of the head. The false caterpillars, or the larvæ of common flies, have only two, like those of bees and the *stratyomæ*. Finally, a great many completely metamorphosed larvæ have no eyes at all.

We may always remark, that the species which exhibit both compound and simple eyes (which has never been observed but in perfect insects) are those which have most need of seeing far, either because, by rising high in the air, it is requisite that they should distinguish their prey at a certain distance, or, because, having large flights to take, they should direct themselves with safety. In short, the insects of a great flight, like birds of prey, have very delicate and very extensive powers of vision.

But in order to give an exact idea of the two kinds of eyes of insects, we shall describe each in particular, commencing with the compond eyes, the organization of which is most complicated, and terminating our description by the simple eyes. We shall afterwards endeavour to ascertain in what way the faculty of vision is executed among this order of animals, when we have followed up all the differences of organization which they present with respect to their eyes.

1. *Compound eyes.* The compound eyes are generally situated on the lateral or middle parts of the head, sometimes even com-

pletely at the base of it, and a few are placed near the antennæ, or more or less laterally and outside of these parts. Placed in orbitary cavities, they are protected not only by the sides of these cavities, sufficiently hard to oppose the impression of external bodies, and by their external membranes, which are almost as hard as the shelly envelope which covers the body, but also internally by soft parts, which seem to consist of tracheæ only. In the animals with vertebræ, the eyes are besides protected by several soft parts like the cartilages, the membranes of the eye-brows and eye-lids, which conceal them under a thick veil, and defend them from external objects. These parts, called by Haller *tentamina oculi*, are totally wanting in insects; and, besides, after knowing the conformation of their eyes, of what use could they be?

We must, however, observe, that some insects present in their cornea, and in general on the furrows which separate the hexagonal facets of this membrane, fine hairs, more or less long, and more or less thick. These have been regarded by Swammerdam and other anatomists as the eye-lashes of the eyes of insects, although they do not seem to perform their office: in fact, they appear to be so little essential, that they are wanting in the greater number; and when they are observed, they are always arranged on the lower part of the eye, a position which is far from being the most favorable to guard it against the admission of foreign bodies. Besides, scarcely any but the most glutinous bodies can adhere to the convex or polished surface of the eyes of insects; and, when they do, the animal can easily remove them with its fore legs. This is frequently observable in bees and flies, which take a pleasure in repeating the operation.

The situation of the eyes, or rather their position, is very variable in the different classes. As this position has a great influence in vision, we shall describe it in the chief families.

The most external membrane of the compound eyes is hard and transparent: it might therefore either be called the *sclerotica* on account of its hardness, or *transparent cornea* from its transparency. This last denomination agrees with it perfectly, for, as Swammerdam remarks, it has the flexibility, the firmness, and transparency of horn. This cornea or sclerotica, convex externally and concave internally, is formed by an infinity of hexagonal facets arranged with the greatest regularity alongside of each other. These facets, divided or separated by furrows which always follow the direction of the cornea, exhibit in some insects, the *hymenopteræ* in particular, hairs which resemble down. As to the lines or furrows of the facets of the cornea, they are curved and a little folded on account of the spherical convexity of the cornea, which interweaves in different places the hexagonal facets with the lines which separate them. In a word, the whole cornea is a true hexagonal net-work, the internal surface of which is divided into as many hexagonal facets as there are at the external surface.

Swammerdam thought that the cornea received some tracheæ, and that these constituted the hexagonal meshes of the cornea. He seemed to think also, that these tracheæ could, in the eyes of the *nymphæ*, serve for their expansion and unravelling. As to the first of these opinions, it does not seem to agree with the organization of eyes in general; for it is very rare that the tracheæ penetrate through the choroid and reach the cornea; and I only know one example, and that is the *libellula vulgaris*. The second fact, being only a consequence of the first, cannot be admitted, if it be true that the tracheæ rarely reach the cornea.

The form of the cornea necessarily determines that of the eye, and seems to have certain relations with the manner of living of the species. Sometimes this form varies in its proportions, and that in the species of one and the same genus; but it seems as if the *sphericity*, or the more or less angular form of this same cornea, is little subject to vary. In general, the cornea is the more spherical, and the more projecting, in proportion as the animal is carnivorous, or when the eye is concealed under a flap of the eye-lid, as we see in the *lampyræ* and others, where the sphericity of the eyes is so considerable, that it comprehends almost the whole head. We may also observe, that the smaller the compound eyes are, the greater is the convexity of the cornea. It is not the same with simple eyes, which in general vary little in point of size.

In order to give a precise idea of these variations, we have described them in a certain number of families, in order that their importance may be known. The cornea is in general cased in a cavity in the hard parts of the head. This union is so complete that it is frequently impossible to separate them: we might even doubt that any separation was possible, were it not for a small ring externally, which marks the line of adhesion. When the cornea is entirely separated from all the parts situated below its internal face, as if from its covering, it seems white and brilliant, which renders it similar to horn. This horn is very thick, in certain species. It is transparent, if we examine it externally; but, when it is not freed from the other parts situated below, it presents either bands or stripes of various colours, or a marbled appearance. It seems even completely black in a great number of species: most of the *coleopteræ*, *hymenopteræ*, and *lepidopteræ*, present this arrangement.

To conclude; this black colour and marble tinge do not, by any means, belong to its texture, but depend, as may be easily ascertained, on the difference in thickness, and the various colours of the humour which adheres to it.

Under the cornea is seen a conduit or pipe, not very liquid, not soluble in water, and strongly adhering to this membrane. Its colour, in general, is between the darkest violet and the deepest black. When this coating presents this colour, which is most frequently the case, it is almost impossible to distinguish it from the humour of the choroid. This is not the case in the eyes, the coating of which is red or green, or of both colours united. We then see very distinctly the coating with its variegated colours, and the humour of the choroid with its black tinge, which never varies. This arrangement is very conspicuous in the *locusta gigantea lilifolia*, the *libellula vulgaris*, and the greater number of the *tabani*. It is also very striking in the *gryllus lineola*, the eyes of which appear to be streaked with brown and green bands, and which is indebted for this singularity to the coating of the cornea being alternately brown and green, and that by nearly parallel bands.

It is, therefore, to the mixture of the tints of the tunic of the cornea, that the variegated colours presented to us by the eyes of insects are owing, precisely in the same way as, from its various degrees of thickness and colouring, the variegated stripes and marble tint arise, which appear at the exterior part of the eye, and which might have been thought peculiar to the cornea. Frequently, as a consequence of this arrangement, one and the same eye presents spots and stripes of various colours, or even one side of a colour totally different from the other.

The coating of the cornea, therefore, covers all the internal surface of this membrane. Its thickness and consistence, like its opacity, are very much subject to variation, as already observed; but it would seem that, in general, the more this tunic is opaque, the thicker and broader are the nervous threads which pass through it, in order, perhaps, that its opacity may not be an obstacle to vision.

We are under the necessity of anticipating a little upon our description, and of speaking here of the optic nerves, which, furnished by the retina, pass through the choroid and its humour, as does the coating of the cornea, in coming into correspondence with the facets of the latter membrane. This arrangement is not the same in the species which present bands or stripes, and in those which want them. If we carefully remove the cornea, and in such a manner as to remove very little of its coating, we observe, in the species in which the compound eye presents stripes externally, this coating to be composed of very distinct rays, one of which is blackish, and the other much deeper, and so on alternately. These two colours are far from being uniform; and, however slightly we look at them, we soon perceive an infinite number of polygons, the centre of which appears to be white. All these white points are the extremities of the nervous filaments coming from the expansion of the grand optic nerve,* or from the retina, and which have traversed the choroid and its coating. We may convince ourselves of this, in the following way: the brain being laid bare, we may follow the optic nerve, and see it directed to the eye, where it spreads, and is divided into a multitude of filaments. But if we draw these filaments, the white points disappear completely, and there only remain on the coating streaks variously coloured, which proves that these white points are the extremities of the nervous filaments. We may recognize this arrangement in the great species, such as the *libellulæ*, the *truxalæ*, and the common cricket. In the species which have no streaks on the cornea, we also observe on the coating of the cornea, the furrows or ridges which form the facets of this membrane, and the coating, which is more or less deep, having the form of a polygon, which represents the facets of the cornea.

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* We designate the nerve which proceeds to the compound eyes under the name of grand optic nerve, in order to distinguish it from the optic nerves which proceed to the simple or smooth eyes.

These retinæ, peculiar to each facet, are those which Swammerdam has called the pyramidal of the eye. These compound fibres, according to him, proceed to a membrane as to a common centre, and it is the circular tracheæ from which the filaments issue. He also observes correctly, that it is through its substance that the tracheæ pass, which ascend along the pyramidal fibres. The figure of these fibres is hexagonal, and their upper extremity is broader than their lower, spreading out a little, as it would seem, when they get into the concavity of the cornea, and taking the form of this membrane.

To conclude; Swammerdam states that he never was able to ascertain whether these fibres were muscular or nervous, although it would have been easy to convince himself of it, since they may be followed to the brain. As to the transverse fibres which Swammerdam has described,* I never perceived them. I am led to think that this expert anatomist must have made a transverse section in the eye and the brain, and that he formed these fibres by the section which he made in the optic nerve. At least, it is certain that these fibres communicate with the brain, for Swammerdam himself admits it; he remarks even that we might compare them to a very considerable nerve which we observe in the furrow, which nerve derives its origin from the brain.

In order to observe these white points, or the extremities of the nervous filaments which compose the particular retinæ of each facet, the cornea must be carefully removed, operating its section from the outside to the inside, and taking care not to remove the coating; for, however little we disturb the filaments, they are so contractile, that they close in upon the optic nerve, and no longer appear on the coating of the cornea. If, instead of dissecting the eye from the outside to the inside, we observe it by successfully removing the internal parts, we can never discover this arrangement, even in the genera where it is most decided, such as the *gryllus*, the *mantis*, the *libellula*, and the *tabanus*. This depends on the contractibility of the optic nerves, which is sometimes so great as to draw the filaments even beyond the choroid.

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* Biblia Naturæ, tab. xx. fig. 5.

Immediately under the optic filaments and the coating of the cornea, we observe the coating of the choroid. This coating or varnish is a viscous substance, not liquid, soft, very clammy, and not very soluble in water. It is also strongly adherent to the membrane which it covers, when the latter exists. Swammerdam remarks, with great truth, that this opaque varnish stains the fingers like the common *pigmentum nigrum.* But this accurate observer seems to have confounded the coating of the cornea with the varnish of the choroid, when he says, that the latter is variously coloured, according to the species. On the contrary, the black colour, and the opacity of the varnish of the choroid, are established beyond contradiction. This colour even seems to be internally connected with the texture of the choroid, for it is impossible to discharge it by repeated macerations. Contrary to the opinion of Hooke,* Swammerdam thinks that there does not exist in the eyes of insects any humour properly so called; and, in fact, neither the tunic of the cornea, nor the varnish of the choroid, deserves the name of humour, particularly if we compare them to what is understood by the term humour in the eyes of vertebral animals. Finally, the same anatomist regards the blackish varnish of the choroid as the extremity of certain fibres placed immediately under the cornea, fibres which have been torn on removing this membrane. This explanation appears very improbable, since, when the tunic of the cornea is of a colour different from the varnish, there never exist any blackish fibres corresponding with this same cornea. Besides, how could the colour of this varnish be always the same with that of the choroid? This single fact demonstrates, in my opinion, an analogy between the choroid and its varnish: it would even seem that the latter is produced by a kind of transudation which takes place through the meshes of this membrane itself.

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* Hooke and Boyle were the first who maintained that air was necessary to combustion and respiration, and that those operations consume but a certain portion of it. Hooke even conjectured that air was fixed in nitre, and that combustion was a chemical process, i e. the solution of the burning body in a fluid, or its union with this substance. The chemists of the present day hold no other language.—*Micrographia*, pp. 45, 104, 105.

If the varnish of the choroid be formed by a transudation of this membrane, it is evident that this varnish ought always to cover it, and this is precisely what is observed. But this varnish, as well as the choroid itself, does not always exist: it would seem even that all the species which shun the light are totally deprived of it. At least I have not observed it in the *blaps*, the *pedinus*, and most of the *tenebrios*: it is wanting beyond doubt in the *blattæ*, which, as is well known, are stupefied with the light of the sun. When the varnish and the choroid exist, it is not very difficult to separate this membrane from the varnish. It seems, then, to be most generally cellular; and in certain species, the longitudinal fibres of which it is composed, are sufficiently decided to make it appear slightly streaked, on account of the tracheæ which are distributed over it: this is distinctly observable in the *truxalæ*. This arrangement has been well described by M. Cuvier, in his Memoir on the Nutrition of Insects, p. 42, tab. i. fig. 3. This organization might induce a belief that the choroid is formed by the prolongation or union of the small tracheæ furnished by the large circular tracheæ: at least, all the tracheæ which proceed to this membrane are totally lost in it; and, as we cannot recover them, after they reach it, we ought to regard them as contributing to form the choroid. This membrane is, therefore, composed of a cellular texture tolerably close, on which there exists a collection of tracheæ furnished by the large circular trachea, and which are imbued with, or rather deeply penetrated by, a blackish varnish. The choroid is more or less black, but always opaque, and its sombre tint is as constant as its opacity. Finally, the longest macerations cannot make it lose its colour or opacity, as we have already observed.—*London Med. and Physical Journal.*

A Practical Essay on the Diseases of the Vessels and Glands of the Absorbent System; being the Substance of Observations which obtained the Prize for 1812, *offered by the Royal College of Surgeons in London: To which are added, Surgical Cases, with Practical Remarks.*

BY WILLIAM GOODLAD, Surgeon, Bury, Lancashire.

We consider the examination of the structure, functions, and diseases of particular organs, in a connected and systematic manner, as certainly leading to the improvement of our science. They mutually throw light upon each other; and, what is perhaps of equal importance, they prevent the promulgation of erroneous doctrines, which might naturally arise from considering them exclusively in one point of view, or from a single apparently accurate observation, when directly contrasted with the results of varied and general experience. The profession is, therefore, highly indebted to the London College, for the selection of such subjects as prize questions; for good must arise from directing the attention of many to one point, even though no single essay should fulfil our expectations. Mr. Goodlad treats in succession of the anatomy of the absorbent vessels, of the chemical properties of chyle and lymph, and of the diseases of the absorbent vessels and glands; to which he has added some surgical cases, with remarks. The two first chapters we shall pass over without further notice. The absorbent vessels may become inflamed, varicose, ruptured, wounded, or obstructed. Mr. Goodlad treats in succession of these diseases, and of their treatment; and, so far as the subject is known, he does it ably and distinctly. It must, however, be obvious that, as absorbents exist every where, they must always be involved in the diseases affecting any part of the body. Thus we cannot conceive any considerable wound, which shall not open lymphatic vessels as well as veins, or any local inflammation in which they shall not be concerned. But cases in which the lymphatics are chiefly, or solely affected, are of rare occurrence, and difficult observation. We have, however, seen a very trifling ulcer of an extremity prove exceedingly troublesome and untractable, by the great quantity of lymph which it discharged, leaving no doubt as to its source. There is also a very singular case related by Mr. Maxwell, in the fifteenth volume of the Medical Commentaries, which most probably arose from a rupture of a lymphatic trunk, although the dissection of the patient did not clear up the nature of the disease.

The diseases of the absorbent glands, of which our author treats, are inflammation, ossification, and scrophula; the last subject, in particular, he has discussed at great length. He divides scrophulous affections of these glands into three *species*: In the first, and more frequent species, an early effusion of fluid is perceptible, which is exterior to the body of the swelled gland, and is contained in, and circumscribed by, the adjacent cellular membrane; the second is attended with simple enlargement of the glandular substance, or of the adjacent parts; in the third species, which rarely occurs, the abscess is formed in the substance of the gland, and if the cells of the gland are separated by adhesion, each cell may contain an abscess, and successive openings are formed for their discharge.

We pass over our author's remarks on the theory of scrophula, and his strictures upon the opinions of preceding writers, with merely observing, that he considers scrophula as a simple chronic inflammation, having nothing specific in its nature, and independent of any morbid change of the fluids. His treatment of the disease accords with this theory; and his directions, upon the whole, seem so judicious, that we are tempted to condense them. To prevent the occurrence of the disease in children, whose frames are weak and irritable, he recommends attention to the strengthening of the constitution, and the removal of any cause of inflammation, by a due use of animal food, the abstaining from hot slopping liquors, a proper attention to the bowels, warm clothing, moderate exercise, plenty of sleep, with light covering, and the shower-bath, succeeded by friction. If any enlarged gland has already appeared, a fur of any kind may be worn round the neck during the day, and a hare's skin fastened to the night cap, and fitted to the part, may be substituted in the night, and worn with the fur side in contact with the skin.

Cooling washes, and the early and frequent application of leeches, must be combined with the general treatment, as his object in this stage of the disease is to diminish and remove the inflammatory action. The surgeon must, however, be upon his guard, not to push these remedies too far. The cooling washes are intended to subdue increased arterial action, and to regulate the temperature of the part; not to reduce it below the healthy standard, or to induce shivering. It is by their continued use, that they will take off the disposition to enlargement. Too great a degree of cold will derange the constitution, especially in weak habits; and if the glandular enlargement be considerable, destroy the power of the part, so as to give it a tendency to slough off. For similar reasons, the local blood-letting must never be excessive, though frequently repeated.

When the progress of the tumour is arrested, we must attempt its absorption by stimulating applications, care being taken that reaction is not produced. This object is well attained by the use of a solution of sulphate of zinc, in the proportion of half a drachm to three drachms in eight ounces of water, which, along with the cooling effects of evaporation, keeps up a gentle stimulus on the surface, using at the same time regular friction with the bare hand, or with some stimulating liniment, and sometimes electricity, along with small doses of calomel night and morning. If there should appear the least disposition to enlargement, the stimulating plan must be suspended, and recourse had to the application of leeches.

"Should the abscess increase, notwithstanding our efforts to prevent it, the glandular swelling with which it is accompanied, will regulate its future treatment. When it is formed of simple effusion, and is uncombined with enlargement in the gland, or when that enlargement is not considerable, the most beneficial practice will be, I believe, to evacuate the matter: for if we wait until the skin be ulcerated, the cellular membrane, having been so long exposed to diseased action, will ulcerate or slough, and the death of the skin must inevitably follow.

"As soon as we are convinced that the abscess will continue to enlarge, it appears proper to puncture it, because the adhesions by which its boundaries are circumscribed are so light, that a trifling pressure destroys them. The abscess extends laterally, therefore, instead of approaching the surface; and as the whole cavity sloughs on exposure, the isolation and death of the skin, and the size of the ulcer, will be proportioned to these circumstances.

"By an adherence to this practice, a further and more important advantage may be obtained, I mean the prevention of ulceration in the cavity of the abscess, and its obliteration by adhesion. Because, though I believe inflammation exists in every period of the disease, yet it cannot be denied that it becomes more considerable as the ulcerative action is more extended. In proportion to the inflammation, is the chance of destruction in the cellular membrane; the matter should, therefore, be evacuated before this increased action has taken place, and whilst the cells retain sufficient vigour to support their life, and to produce re-union. It is true, that in whatever manner, or at whatever time, the abscess is opened, ulceration will follow; but if the inflammation has not been considerable, it may generally be prevented from increasing, by continuing the cold application, after the abscess is evacuated.

"I know not what opinion may be formed of this theory, but the benefit of the practice I have often witnessed, and I can therefore confidently recommend it. An abscess which, if allowed to ulcerate, would occupy many weeks or months, may by this treatment be obliterated in a few days. It is not, perhaps, easy to account for the difference in the effect of the same application, when applied to an abscess with the skin entire, and to the cellular substance, in which the abscess is formed: in the one case, it shall not arrest its progress; in the other, the discharge will immediately cease.

"My first trial of this treatment was in a girl about twelve years of age, who had a small abscess between the metacarpal bones of the little and ring finger, on the back of the hand. It had been preceded by inflammation, to which cold washes were applied, and when the abscess had approached the skin, it was punctured; the cold stupes were continued, and in three days the abscess was obliterated."

Mr. Goodlad is so confident of the success of the sulphate of zinc in solution, for procuring the obliteration of scrofulous abscesses by adhesion, that he does not hesitate to declare, that he should consider a surgeon unpardonable who had an extensive scar from them, when unattended with glandular enlargement.

In one case when his favourite remedy was applied in solution, "the aperture healed, but the abscesses filled, and its contents were repeatedly discharged.

"An ointment of tartarized antimony was well rubbed upon the surface, but though it excited considerable irritation, the contents of the abscess were not at all reduced, nor did it approach the surface. On being again punctured, a strong solution of the sulphate of zinc was injected into it, and retained about ten minutes. It excited little uneasiness, and the parts united by adhesion."

The cases added to the prize-essay, are the case of inguinal aneurism, which the author did us the honour to publish originally in our Journal; a case of gunshot wound, in which the ball passed between the femoral artery and vein, where they pass out of the pelvis, and a cure was performed without any operation, or unpleasant consequences; two cases of congenital hernia; and a singular case, which Mr. Goodlad supposed to arise from inflammation of the theca of the vertebral column.

Edinburgh Medical and Surgical Journal.

ON THE GOOD EFFECTS OF COLD APPLICATIONS TO ULCERS.

BY MR. P. JOHNSON, *Surgeon, Royal Navy.*

GENTLEMEN,

HAVING seen much of the good effects of cold applications to most kind of ulcers, during my service as an assistant-surgeon in a large ship; and having often observed no good effects to have arisen from a general use of ointments to sores, from which there existed any discharge of ill-conditioned pus, and that partook of the ulcerated action, as soon as I was favoured with an opportunity to follow the practice of my worthy surgeon, I lost no time whenever I could make use of the cold applications, to put them in practice; and notwithstanding the high terms in which Dr. Cassils has spoken of the sulphate of zinc, in the following case of scrofulous ulcers, I had recourse to cold sea-water in preference to his solution.

On having joined my ship, about five weeks since, I found one of her company with five deep and high-edged ulcers, situated between two and seven inches above the patella, which affected him for *many* months ; some of them healing, while others were suppurating. My predecessor had used every means that he could devise for their cure, with very little good effect. The man had no constitutional appearances of scrofula, though these ulcers most strongly appeared to partake of that disease. The discharge was ill-conditioned ; the ulcers communicated often with each other, as easily ascertained on pressure, or by the probe. Having seen no written document of my predecessor's treatment, I immediately began with poultices of oatmeal, moistened with salt water, to be changed whenever they became dry, with a little lint underneath. From the 21st January, I used the salt water, by applying a cloth and bandage continually kept wet, the water being every hour changed for more drawn up along-side, up to this day, a period of 29 days, when the ulcers are healed, and the man capable of doing his duty. I have made use of no dressing between the cloth and sores. The cloths were rinsed three or four times a day, consequently very clean.

I must observe, that the surface for the absorption of the saline particles of the sea water was considerable ; being the posterior and lateral parts of the thigh, for the length of eleven inches, as well as where the disease was situated. During these 29 days, he slept with his knee uncovered by the bed-clothes, and most of his thigh too, so that the wet was nearly continual in this time. During my service at sea, I have seen many cases of the sloughing and contagious ulcers which were often cured, and always relieved, by a slight pledget of tow, and only one roll of bandage, loose or tight, as the irritable state of the limb or sore would allow, always wetted with cold water, sometimes fresh, often salt. The acrid and corrosive discharge was by this means washed away, the water being poured on the ulcer, so that there was almost a continual dripping from the dressings. On their removal, the sores looked clean, and when compared with those to which cinchona, chalk, poultices, ointments, &c. were applied, the difference in healthy and clean appearance was great indeed.

Edinburgh Medical and Surgical Journal.

REVIEW.

Medico-Chirurgical Transactions, published by the Medical and Chirurgical Society of London. Volumes third and fourth. London: printed for Longman, Hurst, Rees, Orme and Brown, *Paternoster-Row*, 1812 and 1813.

[Concluded.]

THE first paper in the present volume, contains "Observations on the Venereal Disease in Portugal, as affecting the Constitutions of the British Soldiery and Natives. By WILLIAM FERGUSON, Esq. Inspector General of Hospitals to the Portuguese Army." The author confirms the accounts that have been given of the horrible ravages that are committed by this disease in the British army, and says, that it is probable that among them, "more men have sustained the most melancholy of all mutilations, during the four years that they have been in Portugal, through this disease, than the registers of all the Hospitals in England could produce for the last century; while venereal ulceration has not only been more untractable to the operation of mercury, than under similar circumstances at home, but the constitution, while strongly under the influence of the remedy, has become affected with the secondary symptoms, in a proportion that could not have been expected. With the natives, on the contrary, the disease is very mild; curable, for the most part, by topical treatment alone, or wearing itself out, when received into the constitution, after running a certain course, (not always a very destructive one,) without the use of any adequate mercurial remedy. After illustrating these remarks by facts and observations, that have come under his own notice, the author draws the following conclusions:

"1st. That the disease, in its primary state, is curable in Portugal without mercury.

"2d. That the antisyphilitic woods, combined with sudorifics, are an adequate remedy for constitutional symptoms; the quantity of mercury being always insignificant, and often altogether omitted; or,

"3d. That the virulence of the disease has become so much mitigated, by reason of general and inadequately resisted diffusion, or other causes, that, after running a certain, (commonly a mild) course, through the respective orders of parts, according to the known laws of its progress, it exhausts itself and ceases spontaneously." The fact of the comparative mildness of this disease among the natives of Portugal, is one of the most curious in the history of syphilis, and the statement of its spontaneous cure, is in direct opposition to our former knowledge on the subject. We cannot, however, fully agree with the author in the theory that he has advanced, though we are aware, at the same time, that it is much easier to raise objections to his opinions, than to state others that are more plausible. Experience does not justify us in believing what the author wishes to establish, that syphilis and small-pox, become mild in proportion as they are not resisted; on the contrary, we believe, with regard to the latter disease, that it is more fatal in the natural way, than when the ordinary means have been used to arrest its progress. A great prejudice has always prevailed in Ireland against inoculation for small-pox, and we no where find, even at this day, the violence of the disease more strongly marked, than on the inhabitants of this country. If this opinion militates with experience, it is in our estimation equally opposed to sound reasoning. Who would conclude, à priori, that a disease would become mild, as no attempts were made to resist it, that it would be disarmed of its fury, in proportion as it was suffered to triumph. We refer here, only to the diseases of specific contagion, the laws of which are unlike those that regulate the progress of epidemics. The illustration that he has drawn from the vegetable kingdom, has, in our opinion, no very direct bearing on the question. "The same species of seed," says he, "may be sown upon the same ground, until it shall so degenerate in point of vigour, as to become almost incapable of reproducing itself; but let it be changed to any other kind, though even of far inferior quality, it will immediately display new powers of life, and fructify and vegetate with its native strength." This is true, and it is true also, that in the repeated attacks of the same disease upon the same constitution, each subsequent one is usually milder than the preceding. But we do not see why syphilis should be mild in one constitution, because it has been suffered to pass unresisted through another, or when a second is inoculated why that may not be considered a new soil, favourable to the developement of the disease.

In order to account for the violence of the disease, when carried to foreign countries and when it attacks the British and other foreigners in Portugal, the author resorts to a theory founded upon the imperfect analogy quoted above, viz. that the disease being transplanted into a foreign constitution, has the power of exciting actions of more than ordinary violence. He suggests however, with much more probability another reason why the British have suffered so much from syphilis in Portugal, which is, that at the moment of infection, they are from excess in living, predisposed to disease, and consequently the symptoms appear in them in the most aggravated forms. From what has been stated we are not induced to believe, that this disease is any thing else but common syphilis, with an unusual tendency to excessive inflammation and sphacelus. We confess we are unable to account for the cases of spontaneous cure that are related, nor do we see any satisfactory reason, why the disease does not appear with more violence among the natives of Portugal, except it be, that their low and scanty diet may disarm it of its inflammatory symptoms. The author admits that even among them it sometimes appears, in the most dreadful forms, and this admission, we think, tends not a little to invalidate his theory. We have stated these objections, not only because we believe that some of his opinions are incorrect, but because he has given by them, no doubt unintentionally, countenance to the popular prejudice against inoculation for small pox, and the safer remedy of vaccination. At the same time we think, that the paper contains many valuable and curious facts and will be found extremely interesting.

The second and sixteenth communications are from Dr. Edward Percival of Dublin. The first contains an interesting case of paralysis of the face in a female of seventeen, followed by a long train of nervous affections. The patient was cured, by paying strict attention to the state of the bowels, and employing such topical applications as the urgency of the symptoms required.

The second paper is the history of a fatal case of cynanche laryngea. The symptoms were similar to those described by Dr. Farre in the last volume, but so violent that the most copious depletion did not alleviate them. Upon examining the body, a large abscess was found in the larynx.

Mr. John Mitchill has given in the third paper "an instance of spasmodic affection of the tongue and mouth, successfully treated." The patient was a female about fifty years of age. The spasms came on at intervals and increased in frequency and violence, notwithstanding the liberal use of calomel, iron and different antispasmodics. On examining the mouth, several teeth were found carious, these being removed and the gums scarified, the patient in the course of a fortnight, was restored to health.

Dr. Colin Chisholme has communicated an account of the beneficial effects of mercury, in two cases of severe affections of the brain. In both instances, ptyalism was produced, before a cure was affected.

Dr. Bostock of Liverpool has furnished the fifth and eighth papers. The first of these is an analysis of the bones of the spine, in a case of mollities ossium. By this it appears "that the diseased bone in question contained in one part one fifth only, and in another one eighth of its weight" of earthy matter, whereas human bones in a healthy state contain considerably more than one half.

The second paper is "on the nature and analysis of animal fluids," and most of it was written, the author observes, before he had seen the valuable paper of Professor Berzelius on the same subject, mentioned in our last. It is gratifying to see the zeal with which so accurate a chemist as Dr. Bostock pursues this interesting subject, and the proof which his labours give of the correctness of Dr. Marcet and Professor Berzelius adds not a little to their high reputation. We should find it impossible to give an abstract of this communication, that would afford any gratification to our readers, we must therefore refer them to the original work.

Mr. Thomas Martin has related a case of the good effects of arsenic in chorea. After using cathartics and other remedies without effect, he gave five drops of Fowler's solution three times a day, increasing the dose one drop every day, until it was augmented to thirteen, when it began to disagree with the stomach and the dose was diminished to ten drops. The symptoms soon gave way and the patient, who was a girl of fourteen years of age, was at length completely cured.

"An example of symptoms resembling the tic douloureux, produced by a wound of the radial nerve." is given by Alexander Denmark, Esq. It was first proposed to cut down and remove a portion of the nerve, but as the arm was much diseased, it was concluded, that amputation should be performed; which being done, all the distressing symptoms immediately disappeared, and in three weeks, the patient was entirely well.

Sir Gilbert Blane has furnished an extremely interesting paper, "on the comparative prevalence, mortality and treatment of different diseases;" illustrated by abstracts of cases that occurred in his private practice and in St. Thomas' Hospital. He has divided these diseases into four classes: 1st. Those which have existed and disappeared; as leprosy and the sweating sickness. 2d. Those which have appeared at some period of the world and still continue; as all those arising from specific contagion. 3d. Those that still exist, but are less violent; as typhus, syphilis, plague, small pox, agues, scurvy and rickets. We confess we are gratified with the opinion that the author expresses, that the comparative mildness of syphilis and small pox at the present day, arises from the means that have been used to counteract their effects, inasmuch as it is pleasing to find the remarks that were made on the first paper in this volume, supported by such high authority. 4th. Diseases that are more prevalent now than formerly, as scarlet fever, consumption, gout, dropsy, palsy, apoplexy, lunacy, and all the diseases of the brain and nerves. The alarming increase of this class of diseases, he refers to the progress of luxury and intemperance. The remarks which he makes on the importance of cleanliness and ventilation in large cities and hospitals, will be found highly valuable. The records of St. Thomas' Hospital show the advantage resulting from attention to these subjects. The annual reports to 1741, state the deaths to be, in proportion to the number of patients about as one to ten. Between that period and the present, the Hospital has been so much improved, that all the wards are now well ventilated. The consequence has been, that "the average for the last fifty years, that is, from 1764, is one in fifteen." At the end of the volume there is a supplement to this paper, on the subject of the increase of deaths from the measles, which Dr. Watt of Glasgow, refers to the diminution of the small pox, which, he thinks, " predisposes the system to be affected by measles in a much milder degree, than when they are caught by those who have not undergone small pox." This opinion has been formed in consequence of an examination which was made of the bills of mortality in Glasgow, before and since the introduction of vaccination, by which it was ascertained that of late years, the deaths by measles had increased to an alarming degree. Our author remarks that " this is too serious an inference to be hastily admitted, but in case the researches of others should confirm Dr. Watt's positions, we ought not to shut our eyes against the light."

James Wardrop, Esq. is the author of the tenth and seventeenth papers. The first is " on the effects of evacuating the aqueous humour in inflammation of the eyes ; and in some diseases of the cornea." A phenomenon, that was first noticed by Dr. Barclay, led him to this practice. It was " remarked, that if the eye of an animal be moderately squeezed in the hand, the whole cornea will instantly become cloudy, and when the pressure is increased, the obscurity also is increased. If it be still more squeezed, the cornea becomes of such an opake, milky colour, that the iris cannot be distinguished through it. He likewise observed the same appearances to be produced from filling the veins with water or quicksilver ; but that, whenever the pressure or over-distending cause was removed, the cornea completely regained its transparency, and appeared as if no such experiment had been made. From this curious phenomenon in the dead eye, it was probable, that in the living one, the transparency of the cornea might vary according to the degree of its distention ; and that, in some varieties of opacities of the cornea, the obscurity might arise from an increase in the quantity of the contents of the eye-ball." He thought therefore that the evacuation of the aqueous humour, would be a powerful auxiliary to purging, blood-letting and the ordinary means that are employed to lessen inflammation, as it produced a sudden diminution in the contents of the eye-ball. A favourable case soon occurred, in which there was not only milkiness and opacity of the cornea, but also an apparent distention of the eye-ball. An incision, sufficient to evacuate the aqueous humour, was made through the cornea, the transparency was in a great measure restored, and the pain and inflammation were immediately removed. From the success of this case, the author was induced to try it "in violent cases of inflammation of the eye-ball, in which the cornea had little or no share in the disease. The evacuation of the aqueous humor in ophthalmia may appear to some, on first considering the subject, a violent and even formidable mode of practice, from the means necessary to accomplish it, consisting in a wound being made in an organ already highly inflamed, and become extremely irritable and painful. But the pain of making a puncture through the cornea is by no means acute, when the cornea itself is not inflamed, or otherwise diseased; and in the operation for the extraction of the cataract, where the cornea is quite sound, I have heard the patients compare the pain in making the incision through it, to a hair drawn across the eye. Though the cornea, in a natural state, has but little sensibility, yet it should be observed, that, if diseased, it becomes the seat of very considerable pain; and when it is inflamed, or when ulceration has taken place on any part of it, the pain of an incision becomes acute; besides, from the irritation which the exposure to light produces in an inflamed eye, and more or less pressure being always necessary to hold the eye-ball steady, the evacuation of the aqueous humour cannot be accomplished, in many cases, without giving a considerable degree of uneasiness. The pain, however, produced by the operation, particularly if it be done with care and attention, soon subsides, and the good effects which quickly succeed, are sufficiently compensate for any uneasiness that it may have occasioned. Wounds too, of the cornea, heal with uncommon rapidity; and I have not, in a single instance where the operation has been performed, been able to detect the smallest vestige of an incision, nor has it ever occurred, so far as I know, that any visible cicatrix remained, even in those cases in which the operation had been performed when the cornea was in a previous state of ulceration." Having thus obviated the objections that might be made to the evacuation of the aqueous humour, the author proceeds to give some account of the manner of performing the operation, and the diseases in which it should be resorted to. "The aqueous humour may be discharged by a very simple operation, nothing more being necessary than to make an opening, with any of the knives commonly used for extracting the cataract, through the cornea of a sufficient size to allow that fluid to escape, and in such a situation that any subsequent cicatrix may not impair vision." The knife should not be withdrawn until the fluid has escaped. The the author then proceeds to consider the diseases of the eye in which it would be proper to perform the operation, and these he illustrates with a number of successful cases. Without making any farther extracts from this valuable paper, we will only remark, that the author thinks that the operation would be justified, in all cases of excessive inflammation, or tension of the eye-ball or where there was danger of a rupture of the coats. He considers it as highly beneficial in all cases of an inflammation of the capsule of the aqueous humour, and he has found it serviceable in two cases of ulcers in the cornea.

The second communication is a "history of a diseased metacarpal bone, removed by an operation," together with a description of the instrument that was used for the purpose of removing portions of bone. The result of the case was upon the whole, favourable, though the finger was of but little use. The author thinks that this or a similar operation, is preferable to amputation, where the disease is confined to one of the metacarpal or metatarsal bones.

The eleventh paper is communicated by Alexander C. Hutchinson, M. D. It is a "case of disease in the brain, produced by external violence." The patient was a sailor, who, six years previous to his death, received a wound on the head from a cutlass, in attempting to board an enemy's vessel. From that period, he had a constant head-ach until about a year before he died, when he became subject by intervals to fits of stupor, and paroxysms, very much resembling epilepsy. Upon examining his brain, after death, a large tumour of a scrophulous nature was found in the middle lobe of the left hemisphere, directly under the place where the wound had been received.

The twelfth and twenty-fourth communications are from the pen of Astley Cooper, Esq. The first is a short history of a case of premature puberty. The catameria first appeared when the girl was three years and a half old, and continued at irregular intervals at the period when this communication was made.

His second paper gives "an account of the Anastomosis of the arteries at the groin," in two persons, in whom the iliac artery had been tied. An elegant coloured plate accompanies this interesting communication.

"Pathological Researches respecting the diseases of the Joints, by B. C. Brodie, Esq. F. R. S." form the subject of the thirteenth communication. He has endeavoured to point out the diagnostic symptoms of Inflammation, Ulceration and Morbid charge of structure of the Synovial Membrane, and Ulceration of the Cartilage of the Joints. There are other diseases, he says, that affect these parts, but these are the most important and most likely to be confounded. He has illustrated the subject with a variety of cases taken from the record of St. George's Hospital.

Benjamin Travers, Esq. has furnished the fourteenth and twenty-fifth papers. The first contains "Observations on the Cataract," and his chief object seems to be, to determine in what cases Mr. Saunders' operation is to be preferred to couching or extraction. Cataracts, with regard to consistency, he says, may be divided into "the fluid or milky, the flocculent or fleecy, the caseous or doughy, and the compact or hard:" In the two first, the operation of Mr. Saunders should be used, but couching or extraction in the two last. In performing the operation for the soft cataract, he passes the needle through the cornea, as it gives no pain, excites "but slight inflammation and ensures the laceration of the transparent capsule."

The second paper consists of "Observations upon the Ligature of Arteries." In the first part, he controverts the opinion advanced by Jones, that the obliteration of an artery will take place, though the ligature be removed immediately after its application. He states, that a number of experiments have been made by Mr. Dalrymple to ascertain this point, and in all of them the results were different from those mentioned by Dr. Jones. He then proceeds to make some remarks on the ligature of the arteries for aneurism, and says, that the causes of secondary hæmorrhage are, "1st. Failure of the adhesive inflammation. 2d. Diffused and extending ulceration. 3d. Sloughing and gangrenous inflammation. The desiderata in the operation for aneurism, as it is at present performed, are to excite with certainty and rapidity the adhesive union of the sides of the artery; to limit the ulcerative process as much, and to complete it as early, as possible." He thinks that the present mode of operating, with the double ligature and the division of the artery, tends to counteract these intentions. A number of experiments which he relates, seem to show, that the single ligature is on many accounts preferable to the operation used at the present time.

The fifteenth paper, is a case of contracted wrist, successfully treated by Mr. Hodgson. The patient was cured by immersing the wrist in warm water, and making gradual extension until the parts were restored to their original situation.

A "case of a doubled encysted tumour, the posterior cyst of which, situated deeply between the eye-ball and the floor of the orbit, was attached to, and partly contained a tooth," is the subject of the eighteenth paper. It was removed by Mr. Barnes, Surgeon of the Eye Infirmary, at Exeter.

Thomas Chevalier, Esq. has communicated an account of "two cases of strangulated femoral hernia," in which the operation was performed. In the first case, which terminated fatally, the stricture was not made by Poupart's ligament, or that described by Hey, but was seated in the mouth of the sac. In the second, "the sac containing the intestine, was included within another sac, into which it had descended, so as completely to fill up the aperture, to which it firmly adhered." The result of the operation in this case was favourable.

Mr. Fryer, Surgeon of Stamford, has given a "case of extravasation of bile into the cavity of the abdomen, from rupture of the liver or gall bladder," occasioned by a violent blow. Forty-seven pints of a fluid, resembling in every respect pure bile, were drawn off by tapping, at four different operations. A short time before the last operation, the patient's stools became of a natural colour, and his health rapidly improved. "From that time there was no accumulation of fluid," and the patient soon recovered. It is to be regretted, that a chemical analysis of this fluid was not made.

The twenty-first paper, is on the muscularity of the uterus, by Charles Bell, Esq. The observations in it are, in general, important, and will probably lead to useful, practical conclusions.

Mr. Stewart has given some "observations on the use of opium in uterine hæmorrhage," during pregnancy. In those cases of flooding, in which the patient is exhausted, he gives a large dose of opium before he proceeds to the delivery, which he repeats at different intervals, according to circumstances. This practice he has found successful in many alarming cases; the dose with which he begins, is usually from fifty to one hundred drops of laudanum.

The only remaining communication is on the vascular appearance in the stomach, by Dr. Yelloly. We shall not detain our readers with an account of this paper, as we have already extended this article, to a greater length than was intended. We cannot close it, however, without expressing the great pleasure that we have derived from the perusal of these volumes, and a hope that ere long, their republication will be commenced in this country.

Petit, Serres *and* Goguyer *on the Entero-Mesenteric Fever.*

Paris, 1813.

We have seen two works published in Paris, in the year 1813, on a disease which had proved very mortal in that city, and to which has been given the name of entero-mesenteric fever. The first of these works is the joint production of M. Petit, Physician in the Hotel-Dieu, and of M. Serres, also a physician, and holding some office in the same great hospital. The second is by M. S. J. Goguyer Laprugne, and is an inaugural dissertation, in which he comments on the opinions of Messieurs Petit and Serres.

The characteristics and the nature of this disease were at first obscure, until very carefully noticed and elucidated by dissection by Petit and Serres.

The persons affected with this disease were, for the most part, young men who had been badly nourished, and who had recently arrived in Paris from the country. Yet the disease was noticed in some cases of a different description; in females, in those who had lived on good food, in old men, in such as had always lived in Paris, and in some who had never resided there at all before their sickness; these last having been brought from the country to the hospital.

The disease was noticed among persons of all temperaments; but it was most frequent and most severe among those who were feeble and worn down.

It occurred at all seasons, but was more frequently mortal when the weather was cold and damp, than when it was warm and dry.

The disease was marked in its early period by a sense of debility, by loss of appetite, by general uneasiness, by irregular paroxysms of heat, and in most instances by a diarrhæa, more or less urgent. After these symptoms had continued for some time gradually increasing, the patients became quite unable to attend to any labour, or to take care of themselves, and then they resorted to the Hotel-Dieu. It was thought that the disease progressed very slowly so long as it was left to itself; but when any active medicine was administered, such as an emetic or a cathartic, the disease was aggravated and accelerated. The same effects were produced by excess in eating or drinking.

When patients presented themselves at the Hotel-Dieu, before the more grave symptoms had developed themselves, they commonly exhibited the characteristics which follow. "Their physiognomy had an expression of sinking and sadness; the eye was dull, the countenance discoloured and livid, especially about the lips and wings of the nose; they preferred lying on the back, and had a repugnance to motion; the skin was remarkable for its roughness and dryness; there was a torpor and inaction in the intellectual faculties, which were however correctly exercised when excited; their answers were slow though intelligent; during the day they exhibited few, or no febrile symptoms, but in the evening and night these were more fully developed, the paroxysms coming on gradually without chills and without sudden augmentation of heat, and accompanied by injection of the sclerotica, and most commonly by delirium; this last symptom, almost always slight, was suspended without much difficulty by addressing to the patient precise questions; the thirst was great, the teeth dry, the tongue superficially covered with a coat of a dark grey colour; the alvine evacuations were liquid, consisting of bile and serous fluid, varying as to frequency and quantity, and always insufficient to account for the general prostration of strength; the abdomen was supple, not at all puffed up, and without any, or with very little pain, except on examination; but when one pressed rather deeply at its lower part, on the right side, between the spine of the ileum and the umbilicus, the patient manifested by his cries the pain which he experienced; the suffering in this case was rendered evident, even without any voluntary expression of it, by the spasmodic retraction of the lips and ala nasi, and by the general expression of pain in his whole countenance."

This is a picture of the disease when in a moderate degree and in its most simple state. When patients applied for relief at this period, the disease commonly yielded without much difficulty, especially if the weather was dry and mild. But when it did not, or when the disease had been more advanced before a proper treatment was adopted, it assumed an aspect vastly more serious.

"The expression of prostration and sadness was much more decided; the general complexion of the face was more sombre and earthy, the cheeks of a livid violet colour; the eye dull and sunken, and always injected; the drowsiness and delirium became continual; answers were obtained with more difficulty, although they still continued to be correct; the skin dry, rough, and sometimes covered with petechiæ; the subsultus tendinum was frequent; the febrile symptoms continued through the day, but were augmented at evening with the other distressing symptoms, and with these harassed the patient through the night; the pulses were frequent, feeble, and easily compressed; the teeth dry and slightly dark coloured; the tongue was covered with a brown superficial coat, which seemed almost as if pulverable, very rarely with a thick, black crust; the thirst very great; the abdomen more painful to the touch, the pain being still in some cases limited to the inferior and right part without swelling, in other cases occupying a great extent, and accompanied by meteorism; the alvine evacuations serous, fetid in most instances, but not in all, occurring frequently; urine much diminished; and a tendency to gangrene in parts excoriated, whether the excoriation had been produced accidentally or by epispastics."

Under these circumstances the prognosis was unfavourable, although sometimes, especially when the weather was good, patients recovered from this state. In these fortunate cases there was noticed a general amelioration of the symptoms, particularly of the local symptoms, the pain and tumefaction of the abdomen; and from the first appearance of convalescence, there was a return of a sharp appetite for food. With these symptoms of convalescence, there took place evacuations of two kinds, which were deemed critical. 1st. The urine became copious, depositing a greyish and powdery sediment. This symptom appeared and disappeared several times during the recovery of the patient. 2d. The skin became soft and moist, and was often covered with a warm and copious sweat. The restoration to health was observed to be much more rapid when this evacuation appeared.

In this way the subjects of this tedious disease were restored at length to perfect health; but the recovery was never rapid, commonly very slow, and was always liable to be interrupted by slight deviations in respect to diet and regimen. Exposure to a cold or damp atmosphere was particularly injurious.

In the fatal cases, and these were the most frequent when the symptoms last described had occurred, the bad symptoms went on increasing with very slight remissions, and those of short duration. The patient sunk gradually and exhausted, after having exhibited the most disgusting symptoms; for the vital functions continued until the powers of life were perfectly worn out; the actions continuing in the trunk, when the extremities were already dead.

APPEARANCES AFTER DEATH.

" The subjects were commonly found, on examination, further advanced in putrefaction, than bodies destroyed by other diseases and under similar circumstances in other respects.

" The brain, the lungs, and the heart were found in the same state as usual after common *adynamic* (putrid) fever.

" On the first view, the abdomen seemed to be in a natural state. Tracing the alimentary canal from its origin nothing remarkable was discovered before arriving at the middle of the ileum. There one began to perceive external spots of an oval form, of the colour of wine,* occupying that portion of the canal which is opposite to its attachment to the mysentery. The number and size of these spots increased in approaching the cœcum, and it was very rare to find any of them on the large intestines. On feeling the intestine, in the parts occupied by these spots, there was perceived a greater thickness than in other parts.

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* Probably of a reddish purple, the colour of common claret.

"On opening the alimentary canal, and examining its internal surface, nothing was discovered except at the parts corresponding with the spots already described. In these parts, the mucous membrane was found affected in a peculiar manner.* The spots were exactly defined at their circumference, and were formed by a slight tumefaction of the membrane, the adjacent parts retaining their natural appearance. These spots were much more strongly marked, were larger and attended with more swelling, and likewise were more numerous in the parts near the cœcum, than in the higher parts of the ileum. At the lower part they were often seen an inch and a half, and sometimes two inches in length, projecting more than a line from the surrounding surface, and presenting a confused mass, so as to obstruct in great measure the cavity of the intestine. In these places there could not be discovered any *valvulæ conniventes;* at the expence, or by the extension of which the diseased parts had been developed. The dark appearance, which was noticed externally in the diseased parts, arose from the colour of these spots seen through the transparent peritoneum.

"Independently of this kind of alteration, there were often found insulated pustules, which varied in number in different cases, and which were scattered here and there over the internal surface of the intestine. These, when examined with attention, seemed not to be different in their nature from the diseased spots before described. They were thought to represent the separate elements of the same spots.

"The state of the mesenteric glands corresponded most commonly to that of the mucous membrane of the intestine. Those which maintained relations with the spots least diseased had acquired some increase of size, but not much beyond that which they commonly have; their texture was firm and they were tinged of a light rose-colour. But those, which corresponded with the more diseased parts of the canal, had acquired a more considerable size, which sometimes equalled that of a nut†; externally they were of a blueish-red colour, but internally of a bright red, as if fully injected, and their proper texture was not easily discoverable.

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* M. M. Petit and Serres have given coloured plates to represent the diseased parts.

† The original does not say of what nut; but by the plate accompanying the description, it would seem that the nut referred to must be about equal in size to that of our common walnuts.

"Such were the two extremes of that which we call the state of engorgement. That state had its intermediate degrees, of which it is easy to form a conception. There is one, which was frequently met with, in which the parenchymatous substance of the gland had acquired a perfect resemblance, in respect to colour and consistence, to the substance of the kidney.

"When the disease had been of long duration, the injury of the intestine and of the glands was not ordinarily limited to a state of engorgement. The parts were then found in some measure destroyed by ulceration. The diseased parts in the intestines were flattened scarcely rising above the surrounding surface, and had acquired a colour more dark and more livid. Upon one or many points of their surface were found small round ulcers, of three to six lines in diameter, the bottom of which was sometimes covered with a thick, black, and sanious coating, and sometimes was quite clean, exposing to view the muscular fibres and the peritoneal membrane in an unaltered state.

"In the same cases the glands of the mysentery were found much less voluminous than in the state of engorgement. Externally they were almost black; and internally they presented to view sometimes a substance of a dull, brown colour, in which there could not be traced any vestige of organization, sometimes a half-fluid matter of a dirty white colour, contained in the external membrane as in a shell, to which it adhered only in consequence of its viscidity.

"The different degrees of organic affection were frequently observed in the same subject, but in all cases the parts nearest the cœcum exhibited the greatest progress in the disease."

Such are the appearances occasioned by this disease as observed during life and after death by M. M. Petit and Serres. It was not however in all cases so simple; for sometimes it was found in combination with affections of other parts, such as inflammation of the stomach and inflammation of the peritoneum.

The account we have given of the disease in question is derived solely from the work of M. M. Petit and Serres. Their descriptions are supported by the detail of a number of cases.

In respect to the nature of the disease and the best mode of treatment those gentlemen entertain opinions which it is the whole object of the dissertation by Laprugne to controvert. It is maintained by Petit and Serres,—1. that the entero-mesenteric fever is essentially and primitively adynamic (putrid,) and that consequently the treatment should be tonic and stimulant; 2. that the fever, or general affection ought alone to receive the whole, or the principal attention of the physician; 3. that the inflammatory affection of the intestines, far from contra-indicating the use of tonics, requires the administration of them.

M. Goguyer-Laprugne controverts these opinions, and endeavours to prove,—1. that this disease consists solely in an inflammation of the mucous membrane of the intestines; 2. that the fever, or general affection is entirely symptomatic, and does not consequently require any consideration in the treatment; 3. that the inflammation in this case properly requires for its cure the use of mucilaginous liquids.

To proceed to the analysis of the arguments on these points would lead us too far, and would not perhaps be very useful. While the French doctors disagree respecting a disease under their own inspection, we shall not presume to decide their disputes on this side of the Atlantic.

If the disease is of a peculiar kind and does not occur among us, yet the statement we have made may be useful by leading the attention of the faculty to inflammatory affections within the abdomen; affections which are too often overlooked, not only when the inflammation is slow and obscurely marked, but also when it is acute and violent. If our brethren would open the bodies of those who die of "bilious cholic," and some other analogous diseases, they would often find reason to justify our remark.

We will add that before reading the works, which have been the subjects of this article, it had often been observed by us that inflammatory affections of the intestines are much more common in and near the right iliac region, than in any other part; and that even when the disease extends over a large portion of the abdomen, it appears to centre and to be most severe in that region. It is true however, that these observations were oftener made in cases, where the peritoneal coat was affected, than where the mucous coat was the seat of the disease.

A View of the Mercurial Practice in Febrile Diseases. By John Warren, M. D. President of the Massachusetts Medical Society, and Professor of Anatomy and Surgery in the University of Cambridge. Boston, T. B. Wait & Co. 8vo. pp. 195, 1813.

[From the American Medical Register.]

MEDICINE, like the other branches of natural knowledge, may with propriety be considered an *experimental* science ; and hence it is obvious, if we ever wish to attain to any thing like certainty in it, that the only principles upon which it can be properly established, must be derived from well-attested *facts*. The process of reasoning by induction, so beneficially experienced in prosecuting inquiries in the collateral branches of learning, will no doubt apply with equal success in medical investigation, and be as frequently followed by results no less salutary. It is therefore with peculiar pleasure we take up the present volume, knowing, as we do, that it is the performance of one who has long held a distinguished rank among the most eminent practitioners of this country, and who, amidst the fatigues of professional avocation, finds leisure sufficient to enable him to record for the benefit of others the important results of his own extensive experience.

Dr. Warren commences with a *prospective introduction*, in which are briefly stated the several subjects which he has attempted to discuss. The work itself is divided into four parts. The first opens with a general history of the mercurial practice, which is followed by observations on quicksilver and its medicinal preparations ; on the operation of mercurials upon the body; on their action as stimulants ; their operation on disease ; on the changes which mercurials effect by their action in the system, and on salivation. Dr. W. seems to coincide in opinion with Mr. Hunter, that mercury creates a new action in the system, or what may be termed an artificial disease, by which the original disorder is either subdued or cured ; that its operation is chiefly that of a powerful stimulant ; and that this effect is produced from the oxygen with which the metal is combined; though he inclines to the belief that the activity of mercurials does not depend upon oxygen alone.

"Mr. Cruikshank, of Woolwich, in March 1795, destroyed variolous virus by oxygenated mucilages.

"Without, however, supposing that oxygen has a specific power over the poison, in these cases its action may easily be accounted for, on the principle of a stimulant exciting the system to defend itself against it, or totally to expel it.

"It is a well known fact in pneumatic medicine, that oxygen, prescribed in small quantities, invigorates the constitution without exhausting the excitability.

"May we not, then, conclude it probable, that mercury may cure contagious fever by furnishing oxygen to the blood and the solids by a sort of digestive power separating the oxygen, and perhaps expelling the metallic part of the oxyde from the surface of his body; and that the success with which it has been used in putrid fevers, in which the deficiency of oxygen is generally, I believe, acknowledged, is derived from its supply.

"Indeed, Dr. Darwin supposes that the sensorial power, which is in fact the same thing with excitability, is secreted in the brain from the oxygen in the blood. If so, the utility of the mercurial oxydes in supplying the mass with that material is obvious; and it needs not to be remarked, how much this circumstance must co-operate with the stimulant effect of the oxygen, in increasing the sum of excitement in the system." p. 26.

Dr. Warren is opposed to the at present fashionable and pernicious practice of inducing profuse salivation in the treatment of febrile and other diseases.

"It is a question of some consequence, whether it is necessary to salivate in order to cure fevers. As the natural effect of this process is to produce indirect debility, it seems to be generally agreed, that in most cases of fever, especially those of the less violent kind, salivation may with propriety be avoided. It is doubtful, however, whether mercury will generally produce its salutary effect in the habit, without stimulating to a degree that shall affect the mouth; but as the soreness of the gums and mouth is the only index of the degree to which the mercurial impregnation of the blood is carried, this mark is usually aimed at, as a proof that the object has been attained." p. 32.

The subjects embraced in part second are, a general description of epidemic fevers; the practice in epidemics with mercury; and observations on the general use of mercury in fevers combined with hepatic and other visceral affections. To his own excellent remarks on these several points, the author has added many others of a practical nature from the writings of Hume, Moseley, Wright, Chisholm, and Rush.

Part third is divided into two sections; on the practice with mercury in typhus, and on the yellow fever as it appeared in Boston. The author considers the various species of typhus which have been noticed by practitioners under the names of jail, hospital, and camp fever, as "totally different" in their nature from the yellow fever, as it appeared in Boston in 1798 and 1802; and in doing this, while he manifests the superiority of his own judgment, he affords, in our opinion, additional proofs (had further proofs been wanting) of the absurd reasonings of those who have so obstinately contended for the unity of diseases, and who, in their sectarian zeal, have confounded all the different kinds of fever, for the purpose of propping up a weak and pernicious theory.

Typhus, in the form of ship fever, was prevalent in the Alliance frigate, upon her arrival in Boston, in 1780: a large number of the sick were immediately sent to the hospital in that town.

"The symptoms," says Dr. Warren, "were entirely different from those of the yellow fever, which afterwards spread an alarm throughout this part of the United States; and the disease doubtless originated from human contagion, commencing on board, from the peculiar circumstances of that ship. In an attendance on a large number of these cases, scattered in various parts of the town, as well as contained in the crowded hospital, destitute of many of those accommodations which, under any other than the existing state of the country might have been afforded, I had the misfortune to be attacked with the disease, and noted its progress with particular interest." p. 93.

Dr. Warren also describes, with considerable minuteness, a number of cases of a typhus with uncommonly malignant symptoms which made its appearance in Boston, in 1804 and 1805.

On the yellow fever of Boston much valuable information is communicated. Dr. Warren asserts that this disorder never made its appearance in that town as an epidemic prior to the year 1798. According to Dr. Warren, no evidence of the importation of the disease could be produced, nor was there an instance of its being communicated to those who removed out of the sphere of its supposed origination. We thus most cheerfully make known the sentiments of the author, though of their correctness, on these two points, we must be permitted to doubt: we wish that an equal degree of candour were manifested by some of our fellow editors under similar circumstances.

"The symptoms of this disease were much in the form of that described by Dr. Rush, which prevailed in Philadelphia, in 1793. The number of the sick has never been accurately estimated; but about one hundred and forty-five persons died. Most of those who were seized within the first two or three weeks died.

"The bodies of several of the deceased were examined, and the liver, lungs, and stomach, were found to have been highly inflamed, the veins being excessively turgid with black blood; and in one instance the thorax contained a large quantity of blood extravasated from the ruptured vessels of the lungs. After these dissections, several of the practitioners in the town, convinced of the congestion under which those organs laboured, adopted a much freer use of calomel than had heretofore been practised; and the success of the method appeared to justify the means. Candour requires us, however, to mention that the greater number of recoveries in the latter part of the season, was not to be imputed solely to this improved mode of treatment, but in a great measure to the mitigated form of the disease.

"Calomel was usually prescribed after bleeding and a purge, in doses of two or three grains every two hours, till the mouth became sore; upon which the disease invariably gave way:—in many persons, however, this could not be accomplished. In some cases of salivation, dangerous hemorrhages ensued." p. 115—16.

The fourth part of this work is devoted to a consideration of the mercurial practice in small pox, measles, the throat distemper, cynanche trachealis, hydrocephalus internus, pneumonia and pleurisy, phthisis, dysentery, and rheumatism. No small degree of merit is due to the practitioners of America for their improvement in the treatment of small pox. The information concerning the sore throat distemper which prevailed in Boston in September 1735, and since that time in several other districts of this country, but more particularly in the eastern states, will be read with satisfaction. We agree with Dr. Warren in his opinion relative to the inefficacy of mercury for the removal of croup: the violence of the symptoms of this disease, and the rapidity of its progress, "are too great to admit of any constitutional change by the operation of this medicine." Though calomel has, for a considerable time, been thought to be the principal, if not the only remedy affording any prospect of success in hydrocephalus internus, yet from some recent cases of this alarming disease, which have fallen under our notice, we are disposed to doubt the truth of much that has been said in favour of the efficacy of this mercurial preparation in this complaint. On the subject of consumption the author offers some judicious practical remarks, from which we extract the following as among the most interesting.

"Dr. Rush has published a number of cases of pulmonary consumption treated by mercury.* In three cases salivation proved a complete cure. Dr. Pfeifer relates also a case of a person in a very advanced stage, cured by the same means. This method has been tried in many instances in this place. In one case, which originated from hæmoptysis, it was not successful. In several, where there was evidence of tubercles, and at length suppuration, it was used through all the stages of the disease, but without advantage.

"The following is the only instance in which I found it useful.

"1805, I visited a lady who for several weeks had laboured under a severe cough, with slight pain in her side. On examination, her pulse was found small and irregular, about 100 in a minute, with short and difficult respiration, stricture over the chest, night sweats, extreme debility, and most of the common symptoms of deep pulmonary affection. Expectoration of matter of a purulent appearance, constant diarrhœa, and extreme emaciation, designated the extreme hazard of the case. She was from home when these complaints first came on, and had taken no medicine till I visited her.

"As her countenance was of a yellowish cast, I gave, the first day, an emetic of ipecac. and an opiate at night. I then gave her the usual expectorant medicines; but for several days the symptoms increased rapidly, and I considered her case as almost desperate.

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* Medical Repository.

"Her diarrhœa and cough continuing, I ordered a pill of one grain of ipecac. and two of calomel to be given every twelve hours. On the third day, after having taken six pills, the mouth became to my astonishment suddenly sore, and on examination I found her breath strongly affected with a mercurial fœtor.*

"The pills were immediately discontinued. For several days the salivation continued to increase, the ulcerations in the mouth became extremely troublesome, the breath intolerably offensive, and the gums, tongue, and fauces so much swelled, as to occasion great distress, and almost to threaten suffocation. The most powerful doses of opium procured little rest at night.

"From this time her cough abated, the night sweats were less profuse, her pulse daily became slower, and, by the time the salivation had subsided, which was not till the end of the fourth week, all her symptoms had completely disappeared. She has had no complaint of any kind since, has recovered her strength and flesh, and at this time is in perfect health.

"In this case, from the colour of the skin, there was much reason to suspect some hepatic affection; and the efficacy of the salivation, which was indeed entirely accidental, seems to afford some grounds for a conjecture, that the consumptive complaints were merely symptomatic.

"It is possible, that some disease existing in the liver, might, from its connexion with the diaphragm, bring on the pain in the chest, and the difficulty of respiration; and from the increased irritability of the coats of the lungs, the quickness of the pulse may be accounted for.

"Whether any affection of this nature might have combined with the diseases cured by Drs. Rush and Pfeifer, is mere matter of conjecture. I have, however, no difficulty in concluding that, in complicated cases of this kind, mercury would be preferable to any other medicine in common use.

"In venereal consumption it may, if seasonably applied, with much certainty be relied upon for a cure.

"In the common tubercular phthisis, the powers of this medicine in promoting absorption might, possibly, be sometimes beneficial; but in the ulcerated state I should much doubt its utility.

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* This is one of those cases in which the necessity of great caution in the use of mercury, from the small quantity which will in some habits produce salivation, is exemplified.

" This stage of the disease is easily distinguishable from the other by the appearance of the expectorated matter, according to the established tests for identifying pus, and the uniformity of the symptoms after suppuration has taken place.*

" So far as the action of mercury may be considered as dependent on its oxygen, it might be suspected to be injurious in phthisis, by adding more of that article to the already super-oxygenated mass. If it is a fact, that the mass of blood in consumption is in this predicament, the objection must have weight.

" It would seem, however unaccountable, that this should be the case under a state of the lungs in which a part of their vessels must be impervious. It would be natural to suppose, that less oxygen would be absorbed, and the whole system would rather suffer from a deficiency, just as in cases of pregnancy, wherein one pair of lungs is sometimes insufficient to oxygenate the whole mass for both mother and fœtus, and abortion is the consequence." p. 168—171.

The introduction of mercury in the treatment of pulmonary diseases, after allaying inflammatory action by venesection and other evacuants, may be deemed one of the most important innovations in modern practice. In directing the attention of practitioners to the use of this remedy in a disease which, upon a moderate computation, may be supposed to augment the whole number of deaths in the United States at least one sixth, Dr. Rush deserves the tribute of gratitude. We shall only add, that our own success in several recent cases has tended very materially to strengthen our opinion in favour of mercury in the treatment of phthisis, after the removal of the symptoms which more particularly characterize its inflammatory stage.

But without further remarks on President Warren's work, we recommend it to the careful perusal of practitioners, confident that it will amply repay them for their labour by the communication of many important practical facts.

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* An opinion has been entertained that a species of consumption, denominated mucous, is of the same nature with gonorrhœa, owing to an irritation on the mucous glands of the lungs, which prevents their due preparation of the mucus: hence the form of this disease, which is so easily cured by mercury, is inferred to be of this description. Richter's Medical Observations, 1794, p. 272.

Reply to "some Remarks" on our Review of Le Gallois on the Principle of Life.

In our first number for the present year is subjoined to the Reviews, a paper entitled "Some Remarks on the Review of M. Le Gallois in the last Journal." These remarks we published with great pleasure, although we intimated at the time that we should offer some reply to them.

In this reply we shall advert only to the principal topics of the discussion; since we are not without apprehensions that some of our readers may grow weary of too frequent references to the same subject.

First, M. Le Gallois speaks of "sensation and voluntary motion" as the criteria of life. Our correspondent, to remove some difficulties, would persuade us that "by *sensation* he (Le Gallois) undoubtedly intended to express that property which Bichat calls *organic sensibility*," and that by voluntary motion he meant "*spontaneous motion*, or *mobility*." To this we might reply that Le Gallois spoke of the functions of sensation and motion, and not of the properties on which they depended; and that no one has ever pretended that there are any *organic sensations*. And in respect to motion we might easily show that Le Gallois meant to state that the motions, of which he treated, were strictly voluntary; and that a great part of the force of his argument rests upon this circumstance. But we will not go into this discussion for two reasons; first, because we would not do so much injustice to M. Le Gallois, to so learned an experimenter and physiologist, and one breathing the atmosphere of modern Paris, as to suppose that he did not distinguish between "*organic sensibility*" or irritability, and *animal sensibility*; in fact we know by his own book that he did understand the distinction commonly recognized on this subject, and it would seem one main object of his labours to show that both properties are derived from one common source, and that they are nearly or quite the same;—second, because we are willing to admit the explanation of our correspondent as giving *his* views of the truth in this case.

Second. As to the fundamental question respecting the unity of the vital principle, we are happy to find the opinion of our correspondent accordant with our own ; but it must be noticed that on this point he allows that M. Le Gallois has not succeeded in the first and principal object which his book proposes. This object was to show *the seat* of the principle of life, which cannot be attained if the unity of that principle be disproved.

Third. Passing over minor points, let us proceed to that question, which is really the most important one in dispute ; that is, whether the muscles derive their power of motion from the nervous system. Certain parts, of which those called muscles are the most distinguished, have a power of performing motions ; a power of contraction, and, as some contend, of relaxation. This power has been long ago denominated *mobility.* The same parts have another power ; that of being excited to motion by the agency of foreign causes comprehended under the name of stimuli. To this last power is properly appropriated the name *irritability*, and it is the same which Bichat calls organic sensibility. Now whenever one of these powers is displayed the other is supposed to be displayed at the same time. It is presumed that motion never takes place except in consequence of the application of a stimulus ; and then we have both properties exercised at once. And certainly without motion does take place, we cannot have any evidence that the irritability has been acted upon.

Now the question is, whether these properties strictly belong to the muscle, being communicated to it first in its formation and subsequently in its nutrition, at the same time that it receives its peculiar structure, and likewise by the same instruments or organs, that is, by the nutritive vessels ? Or whether these properties are received by the muscle from the nervous system, and are constantly supplied to it from that source. Far be it from us to decide positively, that these powers are not derived from the nervous system ; but, if they have been proved to be so derived by any experiments yet published, M. Le Gallois and our correspondent are right and we are wrong.

Such is the question to be examined. As preparatory to this examination let it be remarked, that the burden of proof lies not on us, but on the other party. When we find organs of a particular description exercising certain properties, it is fair to presume, that these properties are possessed by those organs themselves, unless it can be proved that they are derived from other organs. If this be disputed in any particular case, it is the part of those, who make the denial in such case, to prove that the properties in question are derived from other organs than those, in which they are manifested.

Now M. Le Gallois has attempted to show, that the muscles do derive their powers or properties from the nervous system, and he has attempted to demonstrate this in respect to the heart particularly.

If a nerve leading to any of the voluntary muscles be divided, that muscle is paralyzed; that is, its sensibility and the influence of volition upon it are destroyed. There does not, however, take place death in the part; its organic functions go on as before; its mobility continues, as is shown sometimes by convulsive motions, and as may be shown at any time by stimulating the part. So much has long since been known.

In addition it has now been shown by M. Le Gallois, that if any part of the spinal marrow be destroyed in its structure, the muscles to which that part sends nerves will lose their life; or, at least, the circulation will cease in all those muscles, and likewise in their vicinity. Not only the influence of the will over these muscles, but also their own mobility will, in this case, be destroyed. Here the effects are much greater than those arising from the mere interruption of the influence of the spinal marrow. That influence is interrupted as completely by dividing a nerve, so far as regards the parts to which that nerve is sent, as by destroying the structure of the spinal marrow. Why this difference? M. Le Gallois does not seem to have distinctly noticed this difference. If we may be permitted to explain it, we say that in the latter case a violent injury is inflicted on an important organ, an organ with which the muscular parts in question maintain very intimate relations, and that, by the power of sympathy, their organic functions are so affected as that those functions are arrested and the life of the whole ceases. Death is likewise produced in all the parts in the vicinity, from the same cause. A sympathetic affection takes place also in other more distant parts, but of course in a much slighter degree.

This explanation accounts for the difference pointed out above, which the principles advanced by M. Le Gallois do not; and it is free from some important objections to which his hypothesis is liable. For instance, we are authorized by analogy, by a knowledge of the similarity of principles employed by nature in effecting similar purposes, to believe that the property we call mobility in animals is the same, as that possessed by the vessels and some other parts in plants. Now if we suppose this property to be communicated to the parts, which exercise it, from the nervous system in animals, we must suppose that it has a similar origin in vegetables. But a nervous system has never yet been discovered in vegetables ; and there is not any function exercised by them, which manifestly requires such a system. It would seem to be the most fair presumption then, that there does not exist in them any such system. But our correspondent thinks otherwise, and to aid his cause is ready at once to believe that vegetables as well as animals possess such a system, he not being quite ready to follow M. Le Gallois, and to "quit the field of conjecture" and to be "guided by facts alone." He says, that there is a circulation in vegetables without a heart, and assimilation without a stomach. He infers that there may be organs performing the functions of the nervous system, though they have not yet been discovered. Does he believe that fair analogy justifies this inference ? In the two first instances, that of circulation or rather distribution of fluids, and that of assimilation, the existence of the functions is perfectly ascertained. It would therefore be fair to infer, that there exist organs to perform these functions, even though the botanist had never yet demonstrated them. But if the existence of such functions was a matter in dispute, if we could explain all the phenomena noticed in vegetables without admitting the existence of such functions, and if organs destined for their performance could not be discovered, it would not be fair to admit the existence of the functions nor of the organs. Now this is precisely the case as regards the question of a nervous system in vegetables. Let us proceed to inquire how far it is proved, that the heart derives its vitality from the nervous system.

If you destroy the structure of the medulla spinalis through its whole extent in rabbits or dogs, and no doubt if in any other animal possessing this organ, the heart very soon ceases to beat. You may, however, destroy any particular part of this medulla, without arresting the actions of the heart. You may even, in a new born animal, destroy the whole cervical portion, the whole dorsal portion, or the whole lumbar portion, and the heart continues to beat; and this so long that it is allowed that the powers of the heart do not depend on either one of those portions alone.

Further, the effect produced on the heart does not depend upon the extent only to which the spinal marrow is injured; it depends in some measure on the mode in which the injury is inflicted upon this part. "When the spinal marrow is destroyed successively in small portions, and at intervals, a much greater length may be destroyed without stopping the circulation, than would be sufficient to produce that effect, if it were destroyed at once."*

Such are the facts; and they certainly do not, in our estimation, amount to evidence that the powers of the heart are derived from the medulla spinalis; nor does there seem to be any difficulty in explaining these facts in accordance with the doctrine of an independent power in the muscles. A violent and rude injury of an important organ, one which maintains direct relations of structure with all the nerves of the trunk, has the effect to destroy life; and it does so more or less suddenly, according to the extent of the injury inflicted at one moment. It surely is not more extraordinary than the effects of a blow on the pit of the stomach, to which we adverted in the Review, and of which we have a few words to say hereafter. That our explanation is right might, we think, be confirmed by any one who chooses to save another poor animal from a watery grave. Let an opening be made in the cranium and let the structure of the whole encephalon be *suddenly destroyed*, as has been done in respect to the spinal marrow; and then try whether, by artificial respiration, life can be maintained as it is where the head has been simply severed from the body, or as where the brain has been removed in successive slices. In this case the nerves of the trunk would not be injured at their origin as in the case of the experiments on the medulla spinalis; and, as the connexion is not so immediate, the sympathetic effect on the organs of the trunk might not therefore be so instantaneous and powerful as where the injury is inflicted on the spinal marrow. But this we venture to predict, that if such an experiment were tried, the heart would cease to pulsate, notwithstanding artificial respiration, much sooner than when the medulla spinalis is simply divided by a needle. No doubt in such an experiment, hæmorrhage would ensue and modify the effects; but, after all allowances for this accident, the experiment would we think give results in support of our doctrine.

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* Le Gallois.

It seems then, if our views be correct, that the muscles are not dependant for their life on the nervous system; although a violent injury to that system may produce such an effect on the muscles as to occasion the immediate extinction of their vital properties.

The doctrine will be still further elucidated by animadverting on some others of the remarks of our correspondent.

"If it should appear that the vital properties are the results of the exercise of the functions of living beings, to the direct agency of which of them shall we ascribe the production of the organic sensibility and mobility? Not to that of respiration, nor the circulation; and we have no better reason to refer it to the nutritive process, as that ceases when no longer supplied by the circulation. However indirectly necessary these functions may be, we have seen that they are not indispensable at least to a temporary existence of these properties. If, on the contrary, we suspend the nervous functions, they are immediately extinct. If this were only indirectly necessary to their maintenance, as is the circulation, or were they the effects of any other cause, their continuance ought to be equally apparent, after the destruction of the nervous influence, as after the cessation of the circulation. We without hesitation assign that property, which is the cause of sensation to the brain, as its source, because, as soon as the communication between the brain, and a part enjoying that property is interrupted, the property ceases to be present. For the same reason must we ascribe the properties, organic sensibility and mobility to the medulla spinalis, as their source."*

In answer we state. The mode by which life is communicated from parent to offspring we need not now consider. In the living being, when it has acquired an independent existence, there take place an increase and a renewal of parts. In these cases the parts newly added, receive their structure from the extreme vessels, and there is no difficulty in supposing that they receive their vital properties from the same vessels, at the same time. These vessels give a structure to parts different from their own; and they likewise give them properties different from their own. If M. Le Gallois' doctrine were admitted, we must still attribute the growth and renewal of the medulla spinalis, as well as of other parts, to the immediate agency of those vessels; and it does not appear from what other source that medulla could derive its properties. Those vessels are the instruments, and the materials, on which they operate, are the various parts of the living blood.

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* Remarks, &c.

Further, we do not assign sensibility to the brain. If this property has any precise seat, it is in the extremities of the nerves. But, in truth, the sentient parts must maintain a connexion with the mind, and this is done through the trunks of the nerves and the brain. It is, however, very certain that the brain is not the source of the property called sensibility, any more than the trunks of the nerves are such a source. The brain is said in anatomical language *to give origin* to nerves; but there is nothing to shew that it gives origin to their properties.

"It is true that 'severe affections of the mind, and blows on the pit of the stomach,' sometimes suddenly produce death; but in these cases, I imagine rather that a suspension of action takes place in the vital organs, than an annihilation of their powers. A blow on the pit of the stomach arrests the action of the heart probably, by first suspending respiration, and not by annihilating its power of motion."

To elucidate this subject, we refer our correspondent and our readers to the "Preliminary Remarks" to Adams's "Observations on Morbid Poisons," second edition. They will there find the distinction between absolute death, and the mere suspension of the functions, clearly pointed out. They will find, on the authority of Mr. Hunter, that a blow on the pit of the stomach will destroy life; not merely suspending the functions, but occasioning absolute death immediately. This was confirmed in one experiment by Dr. Adams. The immediate destruction of vitality in every part prevented the contraction of the muscles, which occurs commonly at, or shortly after the time of expiration; prevented the coagulation of the blood; left the stomach subject to solution by its own gastric juice;

and, by the immediate destruction of vital affinity in every part, left the elements of the body at once under the control of chemical affinity, so that putrefaction ensued at a much earlier period than after common death.

While referring to these "Preliminary Remarks," we have found in them two experiments very fully illustrating some of the doctrines we have stated above, and which we had written before this reference, not recollecting these experiments. The amount of them is briefly as follows. Two rabbits were killed, one by the poulterer, who dislocated the neck; the other by Mr. Brookes, an experienced anatomist, who divided the spinal marrow in the neck. In the latter case the influence of the brain on the muscles of respiration was destroyed, the vital functions ceased, but absolute death did not immediately ensue. The muscles stiffened, &c. as usual, and the properties of life did not depart at once, but gradually left the body. In the other case, where the injury to the cervical portion of the medulla had been inflicted rudely by the poulterer, inflicted as we conceive by wringing the neck, and thus injuring the structure of the medulla, as was done by Le Gallois in his experiments, there ensued not only suspension of the vital functions, but absolute death in every part. Such is the intricate connexion of the parts in animal bodies by a power, for which we know not any better name than that of sympathy, that a rude injury of one delicate organ may instantly destroy the vital properties of all other parts.

"Much stress is laid upon the suddenness with which the destruction of the medulla is accomplished, but the same ultimate effect takes place with respect to the heart, whether it be done suddenly or slowly. But this ought not to be the case, were sympathy the cause."

The remarks we have already made will sufficiently justify the stress laid upon the suddenness of the injury; and the quotation made from Le Gallois' work in this reply* will show that the fact was faithfully observed by him, although he did not well comprehend the cause.

We shall close our reply by a consideration of one point more. In our review the common opinion was stated that the ordinary muscles of respiration are of a mixed character; and we stated in substance that their actions are usually performed without any effort of volition, and are the result of sympathy between them and the lungs; and that this sympathy is maintained through the *par vagum*, *medulla oblongata*, *medulla spinalis* and intercostal nerves. At the same time these muscles are always liable to the influence of volition, whether for the sake of promoting the function of respiration simply, or for any other purpose. When the medulla spinalis is divided the sympathetic influence is destroyed, and respiration ceases. But here our correspondent fancies that he has detected us in an oversight, and he asks why the same effect will not be produced by a division of the *par vagum*. What does occur when this division is performed? M. Le Gallois gives us the answer (p. 201, translation). As soon as the nerves are divided "the respiration is slow and performed principally by the elevation of the first ribs, and in proportion as it becomes more laborious, every respiratory power is called into action. The animal keeps itself quiet and seems only *desirous* of inspiring as much air as it can." Who does not see in this description a picture of the efforts, daily witnessed by physicians, in cases where the respiration is carried on principally or wholly by the power of volition over the muscles of respiration? Is it not obvious that involuntary respiration ceases when the *par vagum* is divided, the medium of sympathy being destroyed, and that the respiration is then carried on very imperfectly, and slowly, and with great labour and great fatigue *by the power of volition?* In these cases the influence of volition continues to be exercised because the communication between the brain and the nerves, which go to the respiratory muscles, still continues. The effort is made because the animal is conscious of the deficiency in the involuntary actions. But it is made slowly, with labour and with great fatigue, as always happens when we find it necessary to exercise the voluntary powers in maintaining respiration.

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* Page 186.

We are disposed to give full credit to all the statements of M. Le Gallois, whose recent death we greatly lament as a loss to the cause of medical science. We are disposed to appreciate justly the labours he performed and the discoveries he made. But surely there is not any use in the publication of experiments, if all who read them are bound to adopt the explanation of the experimenter. It is certainly possible that he may offer an erroneous explanation of the facts he states, as well as that others may do so; and instances of erroneous explanations made by those who have instituted original experiments and by those who have made original observations, may be found in the history of every age of philosophy. The friends of M. Le Gallois must then excuse us if we examine for ourselves not only the facts he has stated but also the deductions to be made from them.

We must here beg leave to close the consideration of this subject, unless some new facts can be adduced in relation to it. Our readers have the arguments before them and will judge for themselves. In order however to understand the subject fully the work of Le Gallois should first be read with care, and then the papers in this and the two preceding numbers of this Journal, which relate to that work.

INTELLIGENCE.

On Thursday, 26th May, a paper, by Sir Everard Home, bart. on the effect of different Injuries in the Brain upon Sensation, was read before the Royal Society. The attempts to determine the functions of the different parts of the brain not having been attended with success, Sir Everard conceives that it would promote the advancement of physiology if medical men were to collect together, and arrange the effects produced by different diseases or injuries of the brain. The present paper contains the result of his own practice. It is divided into ten sections. 1. On the effect of water accumulated in the ventricles. Water accumulated in the ventricles, even to the amount of 6½ ale pints, does not destroy the faculties, provided the bones of the cranium be not united, and the head enlarge according to the accumulation. A curious case was related of a young man whose head had increased enormously, and who retained his faculties entire, except some inconveniences from the size and weight of the head. He was in his nineteenth year, and the head was 33 inches in circumference. When the bones of the cranium, being united, prevent the head from enlarging, the accumulation of water in the ventricles destroys the faculties, and produces idiotism and death. 2. On the effects of concussion. It occasions nausea and vomiting, giddiness and apoplectic fits, which return at intervals for some time. 3. On the enlargement of the blood vessels of the brain. It occasions violent headachs, watchfulness, and disorders of the bowels. The beating of the arteries of the brain has been supposed essential to the exertion of the senses ; but John Hunter retained his senses after the heart had apparently ceased to beat. 4. On the extravasation of blood. It produces similar effects as the accumulation of water ; coma, nausea, apoplexy. 5. On the effects of the formation of pus. It occasions melancholy, lowness of spirits, and mania, with incessant talking. 6. On the effects of external pressure. The depression of the scull occasions loss of memory, the incapacity of using the proper conversation, &c. all which disappear when the cause is removed. 7. Internal pressure from tubercles produces similar effects. 8. Hydatids in the brain occasion bowel complaints, and a comatose state of the rectum and bladder. 9. Wounds in the brain occasion no symptom whatever, nor the destruction of any of the faculties. When a fungous excrescence of the brain takes place through a hole in the scull, the œsophagus becomes so sensible as to prevent the swallowing of solids, from the pain which they occasion. 10. Injuries of the spinal marrow in the neck, occasion paralysis of all the parts of the body below the injury.

An account of the diuretic powers of the *Pyrola umbellata* of North America, was read at the last meeting of the Medico Chirurgical Society. This plant has been long since used by the Indians, in cases supposed to arise from defective secretion of urine, and promises to be a valuable acquisition to the materia medica. It has the property of increasing the appetite, and has been found serviceable in some cases of dropsy.

The following is said to be a very efficacious application for rheumatism, and is known by the name of Sanchez's Balsam :— Aromatic soap, an ounce ; spirit of lavender, four ounces ; camphor, two ounces ; essential oils of peppermint, canelle, lavender, muscade, quaffle, sassafras, of each fifteen drops ; acetic æther, an ounce. The editor of the *Journale Generale de Medecine*, from whom we take the account, considers the efficacy of the remedy to reside principally in the acetic æther.

We understand a case of the successful treatment of Trismus, has lately occurred at the Westminster Hospital. A robust young man was brought in with his jaw firmly locked, in consequence, as was supposed, of the bite of a dog, between the finger and thumb. It was resolved to try the effect of copious bloodletting. He was bled from the arm till he fainted, which was evidently followed by a relaxation of the jaw. Next day he was cupped behind the ears, and upon the back, till the same effect took place. The jaw now opened almost half an inch, and he was enabled to take some nourishment, but the contraction returned towards evening. The third day he was again bled from the arm *ad deliquium*. The disease was now so far removed, that he was able to make a hearty dinner of boiled salmon, and drink some porter. Hearing some conversation about bleeding him again the next day, to which he was always very averse, having no sense of his danger, he left the hospital without leave. It is known, however, that he perfectly recovered, and is now quite well. He lost in all about sixty ounces of blood. The only medicine he took was a grain of tartarized antimony with two grains of opium each night at bed-time. Though this may not be considered as a very decided case of that species of trismus which has almost invariably proved fatal, it at least affords encouragement to give the same remedy a more full trial than it has hitherto experienced.

A Marvellous Tale of Spontaneous Combustion in a Woman, from Leroux's Journale de Medecine.

Dr. Prouteau, at one of the meetings of the Société de Medecine Pratique, communicated a very remarkable fact of spontaneous combustion. A woman, twenty-eight years of age, excessively fat, and addicted to drinking of spirits, (the doctor could not have hit upon a better subject for this miracle,) was found on fire in her chamber, where nothing else was burning. The neighbours who came in threw water on the body, already deprived of life; and related, that as they entered they heard a noise of something frying. She was lying about three feet from the chimney, the fire of which was concentrated in the hearth. The body left upon the floor a layer of black grease; and a book, in which she had probably been reading, was found untouched by the flame. The face and tongue were entirely consumed and reduced to a coal. Beneath the left breast, which was in part destroyed by the combustion, was an opening three inches in diameter, by which the doctor introduced his hand into the chest, and touched several ribs, which he broke with as much facility as if they were calcined, &c. He concludes his narrative by giving his opinion, that the combustion began in the internal parts, and that the clothes were burned secondarily.

Pemphigus is a disease of somewhat rare occurrence, and we believe has not very frequently been observed to prevail epidemically. Mons. **Petiet** has seen it assume this character in the village of Batterans, containing about 294 inhabitants, without any apparent cause for such an anomaly. It came on with the usual symptoms of fever, in addition to which the patients were affected with general tumefaction, and an insupportable degree of itching. On the third day, vesicles, varying in size from that of a grain of hemp-seed to that of a walnut, appeared on every part of the surface of the body, but more particularly on the abdomen, the arms, and the thighs. They contained a transparent and inodorous fluid, were easily broken, and left a violet brown spot upon the skin, which soon disappeared. In slight cases the fever sensibly abated after the appearance of the eruption, and the cure was effected in a week. In some cases, typhoid symptoms supervened, but the author does not state that the disease terminated fatally in any instance. He gave an emetic on the accession of the disease; and, with respect to the after treatment, he says nothing more than that he employed the Aqua Ammoniæ Acetitis, blisters, diffusible tonics, and camphor, in cases requiring them, but that the emetic and nitrous draughts were generally all that was necessary. Out of 294 inhabitants, 35, principally children, were attacked with the disease.

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The Hepar sulphuris has been successfully applied by M. **Bertrand** to tettery eruptions (*affections dartreuses.*) As the French term *dartre* implies a variety of affections, we subjoin the description of two cases where it was useful.

1st. *Dartre rongeante.*—A woman of fifty-seven, had for seven years past suffered from a continual itching in the external parts of generation. On examination, little ulcerations (*disposées en plaques*) were discovered, from which a corrosive ichor was discharged. A wash was employed, in which two drams of the medicine were dissolved, and afterwards an ointment composed of one third of Hepar Sulph. and two thirds of simple cerate.

2d. *Dartre squameuse* (scaly tetter.)—A woman, three years since, was subject to eruptions sometimes on the back of the hands and feet, and sometimes on the face. These eruptions being principally on the face, their hideous appearance induced the patient to apply for advice. They appeared on the cheeks under the form of thick scales, at first white, then of an ash colour, going successively through all their periods to a state of desiccation, when they fell off to re-appear. Two ounces of Hepar Sulph. were dissolved in a bath for the whole body. The ointment was used as before, though somewhat weaker.

Singular Disease.—In the country of the Nogays, a tribe of Tartars dwelling between the Black Sea and the Caspian, on the south side of the river Kuma, there still exists a very singular disease, which is mentioned by Herodotus, and several of the other ancient Greek writers. Herodotus informs us, that when the Scythians were inhabitants of Asia, they advanced towards Egypt, but were prevailed upon by Psammetichus, king of that country, to desist. On their return through Syria, they plundered the temple of Urania, in the city of Askalon. In consequence of this, the goddess sent a *feminine disease* among them.

Reineggs is the first modern writer who mentions the present existence of this disease among the Nogays, who are at present subjects of the Russian empire. Count Potocki, when travelling along the Kuma, in 1798, met with an old man who had this disease. He informed us that such persons are called Coss; and that the disease is not unknown in Turkey, where those subject to it have received the same appellation. The disease, as far as it has been described by Reineggs and Potocki, is distinguished by the following symptoms. It only attacks old persons. The skin grows wrinkled, the beard falls off, and the person assumes completely the appearance of a woman. He becomes incapable of propagating his species, and his sentiments and actions lose their masculine character. In this state he is obliged to shun the company of men and to associate with women, whom he perfectly resembles. The disease is now rare.

THE Gazette de Santé, dated the 21st of June, contains two cases of Laryngitis, *Angine Laryngée œdemateuse*, *Gonflement ou œdeme de la Glotte*, which occurred in the Hotel Dieu, during a cold, rainy, and very variable season. A young man, convalescent from fever, early in the morning took cold; at noon he complained of general uneasiness, slight chills, and heat in the bottom of the throat ; at four o'clock felt acute pain in the larynx ; *inspiration was performed with great pain, but no difficulty was felt in expiration ;* the voice was shrill and tremulous, deglutition difficult, countenance florid, pulse contracted and frequent. Five leeches were applied to the neck, and afterwards a cataplasm. He was not relieved by these means, had no sleep, and his anxiety was very great. On the following day his symptoms had increased ; countenance was of a livid hue ; *expiration* tolerably easy. The patient held his head backwark, to facilitate *inspiration*, which was performed with *inexpressible anguish ; the pulse was hard, small, and frequent.* Five leeches and a blister were applied. He died at two o'clock. After death, the face, neck, and head, were suffused with blood. On dissection, the *mucous membrane of the epiglottis, glottis, and ventricles of the larynx, were inflamed and thickened.* The cellular texture anterior to and at the root of the epiglottis was so large, that the latter was pushed back toward the opening of the larynx, which was much contracted ; the trachea was healthy. If we had not such frequent specimens of the want of energy in the practice of the French physicians, we should suspect that man of idiotcy who could suffer inflammation of the larynx to run its course without the employment of blood-letting. If any disease require depletion to its fullest extent, it is this in its acute state, which makes such rapid strides towards its fatal termination. The unutterable anguish, suffusion of the countenance, and gasping for breath, ought to be sufficient to induce at least a trial of the remedy, even if the inflammatory character of the disease were not so distinctly marked. We observe in the same narrative, a striking confirmation of the inconsistency of the attending physician. A case of Angina Trachealis (croup,) in an adult, is adduced for the purpose of instituting a comparison between the two affections so nearly resembling each other in name. This case, compared with the former, is not remarkable for intensity of suffering, or great danger ; yet, on the fourth day of the disease, *fifteen leeches, and, on the fifth, twenty more were applied to the throat, with advantage.* Now, if thirty-five leeches are necessary for the cure of a case of *Angina Trachealis*, we are at a loss to conceive upon what principles of calculation our continental friend discovers ten to be sufficient for the cure of that horrid assemblage of symptoms characteristic of *Angina Laryngea*. The reporter very properly recommends bronchotomy in the latter, as the means of averting suffocation. It will be obvious, that if the larynx be obstructed so as to make the ingress of air into the lungs difficult or impossible, this operation is our only resource: our past experience does not indeed warrant an expectation of *cure* from it; but, so long as the opening in the trachea is preserved, the symptoms are very considerably abated, as might, *à priori*, be expected. We submit to the consideration of our brethren in the profession, whether it should not be performed before the disease has arrived at its acme; the suffering of the patient would in a great measure be removed *in limine*, and perhaps a source of irritation destroyed, which may have a tendency to aggravate this disease.

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M. Le Gallois, in the Dictionary of the Medical Sciences, has considered the heart, with respect to its anatomical and physiological characters. After having given an exact description of the position, form, and structure of this organ, with the phenomena of its movements, he has been induced to discuss a question which has frequently excited the attention of physiologists, viz. the unequal capacities of the ventricles. Different explanations had been given on the subject, which were but little satisfactory, when M. Sabatier maintained that this inequality arose after death, from the accumulation of blood which took place in the right cavities at the last moments of life. He supported his opinion by the fact, that individuals who die of hæmorrhage, from a rupture of the vena cava, have the two ventricles of the same capacity, &c. M. Le Gallois, with the view of solving this important question, has instituted many experiments. After cutting away the two auricles, the aorta and pulmonary arteries, at their origin, he filled the ventricles with mercury, and then separately weighed the quantity requisite to fill each ventricle. In every case the right was found to be larger than the left, and the difference was sometimes so great, that it appeared difficult to account for its existence in the healthy state. But, considering that, from a cause analogous to that which produces the rigidity of the muscles subject to the will, after death, the ventricles might be so influenced, and the left being the strongest in muscular structure would be consequently contracted the most. He endeavoured to overcome this rigidity, by kneading the ventricle with his fingers, and he in fact succeeded in enlarging the capacity. Some of the animals whose hearts were employed in these experiments were destroyed suddenly, others from hæmorrhage, and in all the cavity of the right ventricle exceeded that of the left; the only exception was in the rabbit, where the capacity of the left surpassed that of the right, but it is doubtful whether this is a universal appearance. In the fœtus the same disposition exists, weich may be owing to the particular mode of its circulation. M. Le Gallois thinks that the reason of two ventricles of unequal capacities emptying themselves to the same degree during their systole, is owing to a reflux of the blood of the right ventricle into its auricle, and which the arrangement of the valve easily admits; the fact he conceives is incontestible.

M. Le Gallois has also considered the circulation of the blood in the fœtus. According to M. Sabatier, the blood of the two venæ cavæ does not pass together through the foramen, but that of the inferior is directed by the Eustachian valve, and that of the superior enters directly the right ventricle, from whence it passes into the pulmonary artery by the arterial canal. M. Le Gallois is of a different opinion; he conceives the Eustachian valve to be insufficient to perform this function; as, in that case, instead of being placed at the anterior edge of the vena cava and the foramen, it ought to have been at the posterior; that it was sufficiently elevated to cover the greatest part of the diameter of the vena cava inferior, and that it was inclined towards the vein in the manner of an arch, through which the blood of the superior vena cava glides. The disposition of the Eustachian valve appears then more proper to favour the mixture of the blood coming from the two venæ cavæ, than to oppose such an union.

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A LARGE biliary calculus was presented by M. Meglin to the *Athenée de Médicine* of Paris, which was extracted from the body of an old cavalry officer, in whom, during life, no symptoms were present that could lead to a suspicion of its existence. It completely filled the cavity of the gall bladder. The patient died of an organic affection of the *urinary bladder*, which had acquired an immense size, and ascended to the umbilicus. It was several lines in thickness, and its muscular membrane was entirely cartilaginous. During the latter years of his life, he suffered much from difficulty in passing urine. At length an abscess, preceded by inflammatory symptoms, was formed at the bottom of the bladder; when this broke, the effusion of urine and pus in the abdominal cavity produced visceral inflammation, which terminated in gangrene. He now complained of acute and burning pains in the abdomen, flatulency, inextinguishable thirst, and hiccup; his pulse was small, frequent, and contracted; respiration hurried; countenance livid, and extremities cold. In forty-eight hours death put a period to his sufferings.

Six other calculi, voided by a female, long affected with hepatic colic, were also presented, The pain was sometimes so intense as to bring her into great danger, during a paroxysm of most acute insupportable pain, with icterus. Bleeding was had recourse to, after which she took Durand's dissolvent remedy (sulphuric æther and oil of turpentine) for six months. In this time she passed fifteen calculi, and was completely cured. M. Meglin speaks very confidently of the efficacy of the remedy. We believe M. Portal, in his treatise on Diseases of the Liver, speaks with some doubt on this point.

M. Zetherman has given an account of a dumbness occasioned by a calculous concretion, situated on the left of the fræum linguæ, and probably in the salivary duct. The patient was enabled to speak, (at the age of fourteen years,) after having spit out this stone, which was the size of a turkey-bean. The same gentleman found a tuft of hair in a vesicular tumour which he extirpated from the eyelid.

M. De Bierkin had under his care a girl twelve years of age, whose tongue was so large that its point fell upon the chin, causing the eversion of the lower lip, and giving to the anterior teeth an horizontal direction. The deglutition had become difficult, the voice unintelligible, and the larynx projected. The disease had existed from the age of two years, but once was cured by replacing it, (as we suppose, by confining it to its situation for a time). It was at length determined to extirpate it, which was done with complete success; the tongue was pierced by a needle with double ligature in two places; the space between the ligatures was first tied, and then the lateral portions; a part of the tongue was suffered to remain; she recovered her voice and deglutition; the lip retook its primitive situation; and the teeth, after being extracted, were replaced in a vertical direction, and properly secured.

Report of the Progress of the Sciences in France, in 1813.

By J. C. Delametherie.

Chemistry.—In the chemical analysis of animal substances, Berzelius has made great improvements. He has submitted the animal fluids, and particularly the blood, to new analyses. The blood, he says, is composed of two parts; a liquid, the *serum*, and one which is suspended, the *coagulum*. In the animal œconomy, we ought to distinguish three principal substances: *a*, fibrine; *b*, albumen; *c*, gelatine.

The serum, according to Berzelius, is a solution of a great quantity of albumen with a little *fibrine*. Both are combined with soda. It also contains some other saline substances.

The coagulum is the colouring matter. It differs from albumen in its insolubility in serum, and by its colour. The colouring matter amounts to about one-third of the mass; the colour seems to be owing to iron, of which it contains about one third of its weight; but this iron can be separated by combustion only.

This colour cannot be produced artificially by uniting albumen with subphosphate of iron, as Fourcroy and Vauquelin have asserted. Nor is it possible to produce it by uniting iron with soda, as Parmentier and Deyeux have supposed.

We may compare the colour of the blood to the other red colouring principles formed by animals, cochineal, kermes, the purple of the murex, &c.

Four hundred grains of colouring matter, when incinerated, yielded:

	Grains.
Oxide of iron	30
Subphosphate of iron	7..5
Phosphate of lime and a little magnesia	6
Pure lime	20
Carbonic acid and loss	16..5

The serum of the blood gave upon analysis:

Water	90..5
Albumen	80
Muriate of potash, soda, and lactate of soda	4
Soda, phosphate of soda, and a little animal matter	4..1

In speaking of the lactate of soda, Berzelius observes, that the existence of the lactic acid discovered by Scheele had been erroneously doubted by Fourcroy and Vauquelin. The blood containing no gelatine nor earthy phosphate.

The fibrine, albumen, and colouring matter resemble each other so closely, that they may be considered as modifications of one and the same substance; they give earthy phosphates and carbonates of lime solely when they are decomposed.

The author thinks that the phosphate of iron does not exist in the non-decomposed colouring matter, and that which we obtain by incinerating it is a product of combustion.

The colouring matter dried and exposed to the fire in a red-hot crucible melts, swells, and burns with a clear flame; it leaves a pory charcoal, which burns with difficulty. In burning, a smell of ammonia is constantly exhaled, although it had been exposed several times to a strong fire.

This extrication of ammonia from a burning charcoal, and which has been long exposed to the fire, is, according to him, a remarkable phenomenon; hence he concludes this ammonia is a new product.

On the Extrication of Caloric during the Coagulation of the Blood; by John Gordon, M. D. F. R. S. E. and Lecturer on Anatomy and Physiology, Edinburgh.

My friend Dr. John Davy, in his interesting thesis (Quædam de Sanguine Complectens) printed here last month, has alluded to an experiment of mine, relating to the extrication of caloric during the coagulation of the blood, which I have been accustomed to mention in my anatomical and physiological lectures for three years back. In the lectures on physiology which I delivered during the three summer months of last year, the limited period of the course did not permit me to dwell so long on that, nor on many other subjects, as I could have wished. To this, I doubt not, it is to be attributed, that the experiment referred to has been in some degree misapprehended by Dr. Davy; and, if by him, who honoured me with the most flattering attention during the whole of that course, I fear also by many other of my pupils.

The following, however, are the notes relating to this subject, from which I then read :—

" As it is a fact established in chemistry that the conversion of a fluid into a solid is always accompanied with an extrication of caloric, one could have little doubt, even if the fact were not made evident by experiment, that the caloric is extricated during the coagulation of the blood.

" Fourcroy had stated it, as the result of experiments made by him at the Paris Lyceum, in 1790, that during the coagulation of bullock's blood as much caloric was given out as raised the thermometer (Reaumur's I presume,) five degrees. (Ann. de Chimie, t. vii. p. 147).

" But more authority seems to have been attached by physiologists to the following experiment of Mr. John Hunter. Mr. Hunter having suspended a healthy turtle by the hind legs, cut off its head, and caught the blood in a bason. The blood while flowing was 65°, and when collected was 66°, but fell to 65° while coagulating, which it did very slowly. It remained at 65°, and when coagulated was still 65°. From this, and similar experiments, Mr. Hunter concluded that in the coagulation of blood no heat was given out. (Treatise on the Blood, &c. 4to p. 27).

" A very different result, however, has since been obtained by the author of a short article on the blood in Rees's Cyclopædia. Ten ounces of blood were drawn into a wooden bowl, in which a thermometer was held. The temperature of the blood while flowing from the vein was 93°. In six minutes the thermometer had sunk to 89°, and coagulating commenced on the surface. On elevating the bulb of the thermometer to the coagulum on the surface, the mercury rose to 90½° and on again depress-ing it to the bottom of the bowl it sunk to 89°. This was repeated twice, with nearly the same result; and on the third trial the quicksilver rose to 91°. The blood was now coagulated throughout; and, after this, the mercury continued to descend regularly, and was no longer influenced by changing the situation of the bulb of the thermometer. In this experiment it clearly appeared that during the coagulation of the blood caloric was extricated; and in sufficient quantity, at one time, to raise the thermometer 2°.

"As it was desirable to confirm this result, and, of course, that before obtained by Fourcroy, both so comformable to analogy, my friend Mr. Ellis and myself, in presence of Professor Thomson, performed the following experiment, in the month of April, 1810:

"Blood was received from the femoral artery of a dog, into a small glass jar. The temperature of the blood flowing from the artery was 99° Fahr. and that of the apartment, during the experiment, 46° Fahr.

"One minute after the blood had been received into a vessel, it began to coagulate, by a film on the surface. The bulb of a very delicate centigrade thermometer was now placed into the blood at the upper part of the vessel, and held there during a minute, without touching the sides of the glass. It was then depressed to the lower part of the vessel, where coagulation had not begun, and held there in the same manner during the next minute. During the next it was held at the top, and during the next at the bottom; and so on, it was alternately elevated and depressed for twenty successive minutes after coagulation had begun on the surface.

"When the bulb was first placed in the blood at the top, the mercury gradually rose to 34°; but, when it was depressed towards the bottom, it instantly fell to 30½°. When again elevated, it rose to 33½°; and, when again depressed, sunk to 30°. A third time brought to the surface, the mercury rose to 32°; and a third time depressed, it fell, in half a minute, 28$\frac{8}{10}$°. At the next elevation the mercury rose to 31°; and at the succeeding depression, fell to 28½°. At 18¼ minutes after the blood had been drawn, when the bulb of the thermometer was brought from the bottom towards the top, the mercury rose from 24° to 25½°. It was now held at the top for two minutes, and the mercury gradually fell to 24°. The blood seemed now completely coagulated, and the experiment was discontinued.

"In this experiment, therefore, the extrication of caloric during the coagulation of the blood was rendered sensible by the thermometer for twenty minutes after the process had commenced; and was at one period so great as to raise the thermometer, in this cool apartment, 3½° of the centigrade scale, which is equal to 6·3° Fahr.

"That a similar extrication of heat was not apparent in Mr. Hunter's experiment on the blood of the turtle, may have been owing to his not having placed the bulb of the thermometer, alternately in the coagulating and yet uncoagulated part; and partly, perhaps, to the slowness with which he informs us the process went on.

"We may regard it, therefore, as established, that, when part of the blood thus spontaneously passes from a fluid to a solid state, caloric is extricated, in the same manner as when other fluids undergo a similar change."

Farther than this, my notes did not then extend.

But during last winter I had an opportunity of trying this experiment several times, on venous blood drawn from persons labouring under inflammatory complaints, and the result was always precisely similar. The following is a note of one of these experiments:—

Jan. 22, 1814. I received three ounces of blood from the median vein of a man, aged forty, labouring under pneumonia, into a tall glass vessel, and immediately introduced into it a delicate Fahrenheit thermometer, the bulb touching the bottom. The temperature of the blood in this situation was 76°. In two minutes fluid *size* collected at top; and in two more a very thin film appeared on the surface of this size, and the thermometer was then exactly 74°. In four minutes more, that is, eight minutes after the blood had been drawn, a soft coagulum was formed to the depth of an inch from the top and the thermometer (the bulb being still at the bottom) was 73°. I now raised the bulb smartly into the middle of this coagulum, and instantly the mercury rose to 85° (that is 12°); and when I depressed it again to the bottom, where the blood was still fluid, the mercury immediately sunk to 73°. I repeated this several times with similar success. The temperature of the apartment during the experiment was 55°.

I have tried this experiment again, within these few days, on blood drawn from the arm of a middle aged man, labouring under an affection of the heart, and the result was similar.

On carefully reviewing all these experiments, I cannot discover any source of fallacy connected with them, which should lead me to hesitate in deducing from them the same conclusion as formerly, viz. that caloric is extricated during the coagulation of blood, and therefore that the blood is no exception to the general law applicable to all other fluids in this respect.

That my friend, Dr. Davy, has been led to adopt an opposite opinion in consequence of his experiments on lamb's blood, has obviously arisen from his not having been aware of the importance of moving the thermometers in his experiments; a circumstance for a knowledge of which I am myself entirely indebted to the anonymous author already referred to in Rees's Cyclopædia; and a circumstance, I may add, which, unless it be scrupulously attended to, is calculated to render all experiments made with a view to ascertain the temperature of blood after it has been drawn from the vessels, altogether inconclusive. At the same time, I cannot help observing, though with the utmost deference to one so much more familiar with chemical details than I am, that, taking Dr. Davy's experiments as they are, they rather seem to me to warrant an opposite conclusion to that which he has drawn from them; and that, upon the whole, they rather tend to confirm than to disprove the extrication of caloric during the coagulation of the blood.—*Annals of Philosophy.*

Hooping Cough.

M. Bertrand, having vaccinated twenty-one children labouring under the hooping cough, draws the following conclusions. 1. That the progress of vaccination, even in its most perfect state, produces little or no effect at the attack or during the first stage of hooping cough. 2. That the cough is alleviated by vaccination, when the disease has arrived at its second stage. 3. That, when the hooping cough is in its last stage, vaccination produces a remarkable, and, as it were, a specific effect upon it. In the first stage it produced only a slight effect upon one out of nine children. In the second stage the disease was sensibly influenced in five out of seven children; and in the third stage, out of five children, one obtained an immediate cessation of the cough, and three were sensibly relieved during its further progress. On one it produced no effect; but the strength of this child was so reduced, that the vaccine pustule, although possessing its true character, went through its course but very imperfectly.—*Medical and Physical Journal.*

Dr. Whitredge informs one of the editors, that he performed the operation of tying up the external iliac artery for an aneurism in the groin, at Sacket's-Harbour, on the 8th of January, and that the appearances of the patient promised a successful event.

Experiments on Rabbits, the fourth day after birth, to ascertain the comparative effects of dividing the spinal marrow, of decapitation, and of destroying the spinal marrow.

Case I.—Destruction of the whole of the medulla spinalis. Sensation instantly ceased. A thigh cut off at 4′ did not bleed. At 59′ opened the thorax and examined the heart; there was not any motion nor could any be excited.

Same case on another rabbit. Sensation instantly ceased on the destruction of the medulla spinalis. Gapings continued 13½′. Pulsation of the heart could not be felt through the parietes of the thorax. The thorax opened at 40½. The right auricle was found to contract slightly a few times, which contraction ceased at 46½, and could not again be excited. The left auricle and ventricles did not contract in either of the above animals, nor could they be excited.

Case II.—Section of the medulla near the occiput. Sensation continues. No gapings. At 5′ amputated a leg, which began to bleed in one minute after. Pulsation of the heart felt through the parietes of the thorax. At 56′ opened the thorax to examine the heart. The ventricles and left auricle did not contract, neither could they be excited so to do; but the right auricle contracted till 62′, when it ceased entirely.

Case III.—On another rabbit. Decapitation and ligature of the carotids, (some blood lost in the operation.) Gapings and sensibility ceased at 10′. A thigh cut off at 11½′ gave black blood as in the above case. The thorax opened at 42½; the left auricle alone was found to contract strongly, but began to grow weaker at 48½, and terminated at 50′. The spinal canal was examined in the two animals of case first, and the medulla was found to have been perfectly destroyed in its whole extent.

Bell on the Venereal.

A HANDSOME edition of "A Treatise on Gonorrhea Virulenta and Lues Venerea, by Benjamin Bell," with notes adapted to the present state of practice in those diseases, has been published at Albany, by E. F. Backus. This work is well known to the medical public, as containing useful and ample disquisitions on the numerous forms of disease, which appear as symptoms or consequences of syphilis and gonorrhea. Knowledge, however, is progressive in this as in all other departments of practice, and it is to be expected that an edition appearing twenty years after the original publication, must be susceptible of some improvement. The American editors profess to have added in the form of notes such improvements in practice, as well as investigations on the nature of the disease as have been offered since the period at which the author wrote. The notes annexed to this edition are very copious, and contain abstracts of the writings and opinions of Home, Whately, Charles Bell, Howard, Pearson, and other late European authors, as well as of several American physicians and writers of celebrity. In its present state the work will, no doubt, be satisfactory and useful to those practitioners who wish to possess *multum in parvo*, on the diseases of which it treats.

TO SUBSCRIBERS AND CORRESPONDENTS.

THE editors owe an apology to the public for the inaccurate and slovenly appearance of the last number of this Journal. This is to be attributed in a great measure, to the negligence of the printer employed, but partly to the bad manuscripts, which are often communicated to them. The editors are determined not to support a publication, which is not creditable to themselves, as well as useful to the public, and have therefore applied a remedy to the evil, so far as depends on them; and now request those correspondents, who may favour them with their communications, to present them in a form, sufficiently distinct and accurate to prevent the necessity of corrections, as the Editors may not have leisure to make them.

ABSTRACT OF

THE BILL OF MORTALITY,

FOR THE TOWN OF BOSTON,

From the 31st December, 1813, *to the* 1*st January*, 1815 ;—*agreeably to the Record kept at the Health-Office.*

	Males.	*Fem.*	*Tot.*	*under* 1 *y*		1 *to* 2		2 *to* 5		5 *to* 10		10 *to*20		20 *to*30		30 *to*40		40 *to*50		50 *to*60		60 *to*70		70 *to*80		80 *to*90		90 *to*100	
				M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.
January	32	30	62	13	3	4	1	3	1	2	4	0	1	3	5	2	5	2	2	1	1	0	1	1	3	1	3	0	0
February	32	21	53	8	7	6	1	3	1	3	0	2	2	5	2	1	0	0	4	3	1	1	1	0	2	0	0	0	0
March	40	25	65	10	6	4	2	2	0	3	1	2	2	4	3	4	2	4	3	3	1	1	1	2	2	1	2	0	0
April	31	25	56	6	3	2	0	2	5	0	0	3	0	5	5	5	3	2	3	2	2	3	0	1	3	0	1	0	0
May	26	18	44	6	3	2	2	2	1	1	0	0	2	4	3	2	1	1	3	4	3	0	0	1	0	3	0	0	0
June	27	13	40	6	1	2	1	2	1	1	0	1	3	4	3	4	1	6	0	0	0	0	0	1	1	0	2	0	0
July	40	23	63	9	6	5	1	0	0	4	0	3	0	7	5	4	7	6	1	1	1	1	0	0	2	0	0	0	0
August	35	44	79	13	11	4	7	2	5	1	0	1	1	4	7	3	4	3	2	0	0	3	3	0	4	1	0	0	0
September	28	23	51	12	7	4	4	1	1	0	0	1	1	5	5	3	0	1	0	0	1	0	1	0	2	1	0	0	1
October	52	48	100	14	4	12	5	6	4	2	1	0	4	4	6	7	8	4	4	0	6	1	4	1	2	1	0	0	0
November	35	31	66	2	3	2	2	6	3	0	1	4	0	8	11	4	5	3	0	1	0	2	2	2	4	1	0	0	0
December	21	27	48	4	4	2	1	0	2	2	2	2	0	2	4	6	6	0	2	2	0	0	0	1	2	0	4	0	0
	899	328	727	103	58	49	27	29	24	19	9	19	16	55	59	45	42	32	24	17	16	12	13	10	27	9	12	0	1

☞ *The Deaths above were caused by Diseases and Casualties, as follows :—*

Anasarca	17	Croup	2	Fever, Pulmonic	26	Insanity	2	Spasm	1
Apoplexy	3	Debility	1	—— Typhus	77	Jaundice	2	Still Born	32
Burns and Scalds	5	Disorders unknown	43	Fits	11	Mortification	8	Sudden	14
Casualty	3	Drowned	10	Gout	2	Neurosis	1	Suicide	1
Cholera Morbus	2	Dysentery	4	Hepatitis	3	Old Age	39	Synanchia Tracialis	1
Cholera	1	Fever, Inflammatory	3	Hooping Cough	5	Paralisis	9	Teething	1
Consumptions	153	—— Bilious	6	Hydroceph. Internus	3	Phrenitis	1	Tetanus	2
Convulsions	1	—— Puerperal	6	Hydrophobia	1	Quinsy	10	White Swelling	1
Cramp	2	—— Pleurisy	3	Infantile Diseases	208	Scrofula	1		727

Published by order of the BOARD OF HEALTH.

NATHANIEL GREENOUGH, *Secretary.*

BOSTON, *January* 10, 1815.

*** BOSTON lies in 42° 23′ 15″ North latitude, and 70° 52′ 42″ West longitude. The Census of 1810, in the month of August, states the number of Inhabitants at 33,250

THE

NEW-ENGLAND JOURNAL

OF

MEDICINE AND SURGERY.

Vol. IV. JULY, 1815. No. 3.

NEWLY INVENTED TOURNIQUET.

[For the New-England Journal of Medicine, &c.]

Some surgeons consider themselves very meritorious for contriving a new instrument, or altering and distorting an old one. The instrument of which an engraving is given in this number is not to be ranked among these useless novelties. It is a valuable improvement in the surgical art. The tourniquet is an instrument which it is important to have perfect, because its uses are so momentous. The tourniquet of Petit, which is the one in common use, is liable to break when the screw is used with much force; it is quite unsteady upon the limb and more complicated than is necessary. These inconveniencies are remedied in the one represented, which seems to be an instrument of great simplicity, strength, and exactness.

This tourniquet has been invented by Francis Moore, M. D. of Ipswich. It does credit to his ingenuity, and there is no great doubt that he will have the satisfaction of seeing its use supersede that of the common tourniquet. The opinions of some practitioners of its merit are given below, in order that the at-

WOUND OF THE AXILLARY ARTERY.

[Communicated by Thomas Hubbard, M. D. Pomfret, Connecticut.]

January 27, 1815. HARRY CARTER, a black man, aged twenty-six, in the evening in a fracas, received a wound with a butcher's knife, in the posterior and upper part of the left arm. The hæmorrhage was very profuse; medical aid was applied for, but not obtained until the next morning.

28th. Dr. D. Hutchins, of Pomfret, visited the patient. The attendants stated to him, that the patient had lost two gallons of blood. However, by their dressings, assisted by compression, the hæmorrhage had now ceased.

Dr. Hutchins prescribed bark and opium. His feet were œdematous.

February 1. Dr. Hutchins found the arm very painful and excessively swollen. The patient had been unable to lie down since the hæmorrhage had been suppressed, and had spent the greater part of the time standing in a particular posture.

Subsultus tendinum had occurred. A mild cathartic was administered, opium directed.

2d. The pain and subsultus were less. An aneurismal tumour had formed round the wound. The cathartic was repeated, and the quantity of opium lessened.

3d. The aneurismal tumour increased, the arm above the wound excessively swollen; indeed the whole arm was extremely painful, and œdematous to the fingers. Dr. J. Palmer, Jr. of Ashford, attended in consultation. The whole arm was rolled in a bandage.

4th. This morning, coagula of blood burst out at the wound. A violent hæmorrhage ensued, which was suppressed by applying a cord around the scapula and under the axilla, and twisting it with a stick. At noon the tourniquet was removed, and the whole arm loosely dressed.

5th. I attended in consultation with the gentlemen before mentioned. I found the external wound about three inches below the joint of the shoulder, in the back part of the arm. A large aneurismal tumour extended from the wound to the axilla. The whole arm and hand excessively swollen, œdematous, and cold. There was no pulsation in the radial artery at the wrist. The patient was extremely weak, and some hæmorrhage took place while we were examining the wound.

I proposed the ligature of the artery as the only resource, to which the other medical gentlemen readily assented.

The patient was laid upon a table, and the arm separated from the body at right angles. Strong compression was made upon the left subclavian artery, as it passes over the first rib, by an assistant who applied his thumb forcibly above the clavicle. As it was totally uncertain at what precise point the axillary artery was wounded, it was thought most proper to enlarge the original wound obliquely upward till we should be able to discover the wounded artery. I accordingly made an incision in the above direction, through the skin and cellular substance, extending it to the edge of the pectoralis muscle in the axilla. I then introduced the fore finger of my left hand into the original wound, and making use of it as a director for the scalpel. I quickly enlarged the wound in the aneurismal sac, in the same direction as the first incision was made, and turned out the coagula of blood. A jet of blood issued from the artery high in the axilla.

I then introduced my left fore finger under the artery and nerves, and with my thumb compressed the artery, and the hæmorrhage immediately ceased. The assistants then cleared the cavity more completely of extravasated blood. I discovered that it was necessary to enlarge the incision upwards and inwards, and to divide some of the fibres of the pectoralis muscle. I introduced the fore finger of my right hand under the edge of the muscle, and directed an assistant to pass a probe pointed bistoury upon my finger and divide some of the fibres of the muscle obliquely upwards. In making this incision, the cephalic vein was divided. A quantity of very dark blood issued out; but the discharge was soon stopped by compression with the finger of an assistant.

We then discovered the artery wounded high in the axilla, surrounded by the plexus of nerves, none of which were injured. It appeared that the point of the knife, by which the original wound was inflicted, had penetrated obliquely upwards, and divided the artery partially, making a small flap.

With some difficulty, I succeeded in passing a common blunt, crooked needle, armed with a double ligature around the artery. One of the ligatures was tied above and the other below the wound in the artery, and very near the orifice. The artery was not divided between the ligatures.

The assistant who took charge of the compression of the subclavian artery removed the compression, and the artery was seen to pulsate against the upper ligature.

The edges of the wound were now brought as near together by the adhesive plasters, as the swollen state of the parts would admit; over the plasters, lint, compress, and a bandage were loosely applied. Some aloetic pills were directed.

6th. A pupil of mine saw the patient this day. The patient had passed a quiet night, his arm had been much less painful since the operation. The aloetic pills had produced a discharge from the intestines. His feet, face, and affected arm were œdematous. The arm, which had been very cold for a few hours after the operation, had now become preternaturally warm.

7th. I removed the dressings; the tumefaction of the arm had greatly subsided, as well as the œdema of the feet, &c. The arm was of a natural temperature. The discharge was large and serous, of a greenish colour and intolerably fœtid. We *imagined* that there was a small pulsation of the radial artery at the wrist. Appetite good; aloetic pills repeated.

I again dressed the wound; the tumefaction of the arm had still further abated; suppuration abundant, though the discharge was less thin and fœtid; dressed as at first. Bark was directed. The patient was left in the care of the two gentlemen who first attended him.

19th. A student of mine saw the patient, and found that he had been constantly mending, since my last visit. The patient complains of a slight numbness in the arm at times. The attending physicians have thought that they had *occasionally* felt a pulsation of the radial artery at the wrist. An evident pulsation was, however, to be perceived near the elbow, where the anastomosing and the recurrent branches unite.

The attending physicians report that he afterwards gained rapidly; the wound healed fast and he was recovering the use of his arm, when he absconded to parts unknown.

[To the Editors of the New-England Journal of Medicine and Surgery.]

....

[Having observed that cases of a nature, very similar to that of which I have here given an account, have excited often considerable attention, and been investigated with some interest, I thought that the following communication might be deemed of sufficient consequence to find a place in the pages of the Journal.]

JOHN WARE.

Cambridge, June 1815.

CASE OF POLYDIPSIA.

The subject of the following account is a young man, named James Webb, now living in the town of Hingham, in this state. Having been informed of his singular case, I went in company with Mr. Norton, librarian of Harvard University, to ascertain its nature and the truth of the circumstances related concerning him. During our visit the following facts were collected from his own account of himself, which were confirmed by the testimony of the persons with whom he now lives, and as will be presently stated more particularly, by that of others with whom he has formerly lived.

He was twenty years of age sometime in the month of October, 1814. His appearance is that of firm health; his complexion is dark and ruddy; he is short, thick, rather sturdy, and, except his preternatural thirst, has never been afflicted with disease. At present, the quantity which he habitually drinks in twenty-four hours, amounts to three pailsful, or six gallons. This is necessary, not only to prevent the sensation of a tormenting thirst, but to preserve him in his ordinary state of health; for when he abstains from his usual allowance, his head is affected and he becomes dizzy, weak and sick; or, as he himself expressed it:—"When I don't drink, it gets into my head." He finds himself obliged to drink at intervals of about an hour and an half or two hours, one or two quarts at a time. At night he places a pailful of water at his bedside, the whole of which he requires before morning, waking whenever it becomes necessary. He has sometimes taken to the amount of a gallon at once, without experiencing any bad effects. He drank two quarts in our presence, having taken one but fifteen minutes before. He swallows with great eagerness and the appearance of satisfaction, drinking a quart in the time that a common person would a quarter of that quantity. He uses water directly from the well, even in the depth of winter, and avoids mixing any thing with it, especially any kind of spirituous liquors, which he dislikes. Its coldness causes no inconvenience, except occasionally a slight chill. He has no recollection of the time when this habit commenced, but has been told by his parents that it was in infancy and soon after birth. The quantity which he now drinks, does not, he thinks, differ materially from that which he drank at nine or ten years old. He has several times endeavoured to break off the habit, but has always suffered from the attempt in the manner above mentioned. His appetite for food is not remarkable. The persons with whom he lives merely observing, that he was a hearty eater. His meat and drink at meals, are like those of the persons with whom he lives. His pulse, during our visit, was full, strong, and remarkably infrequent, not exceeding, at any time, fifty-six pulsations in a minute, and being sometimes so few as forty-five. It varied as follows:

Some time after drinking, fifty-six.

Fifteen minutes after drinking and just before drinking again, fifty.

Immediately after drinking two quarts, forty-five.

The temperature of the atmosphere has no influence on his thirst, since he requires the same quantity of water in the warmest, as in the coldest weather. He had an uncle who was formerly affected in the same way, although not to an equal degree. He served in the army during the revolutionary war, and was said to have died in consequence of being in a situation where water was not to be obtained.

As would be supposed, these extraordinary quantities of fluid are wholly carried off by the kidnies, and affect the secretion of no other part. His urine, he thinks, equal in quantity to the whole of the water which he drinks. The qualities of the secreted fluid, I had no opportunity of examining. He perspires very little, and his fæces are of the usual healthy consistence.

This account is confirmed by Messrs. Wilder and Hersey, two persons with whom he formerly resided. With Mr. Wilder, he lived some time at about the age of nine or ten years, and his relation agrees with that of Webb in every particular, especially as to the quantity of water drank during twenty-four hours.

With Mr. Hersey, Webb lived from fourteen to eighteen years of age. He thinks that the quantity increased while he was with him, and feels confident, that during the latter part of the time he drank as much as four pailsful or eight gallons daily, and has known him to drink a gallon at a time. He never knew him suffer from want of water but once, when away from home where none was to be obtained; he looked pale and said he could not live ten minutes longer, but was immediately relieved by drinking. He used to shudder for ten or fifteen minutes after drinking, so that his teeth would chatter. His health and appetite were good. He used no spirit. Mr. Hersey says, that he has understood from the person with whom he lives now, that the quantity has diminished since Webb has been with him.

In the second and third volumes of A Collection of Medical Facts, published in London in 1792, are accounts of three cases similar to that above detailed, of which, for the sake of comparison, it may be worth while to subjoin a short notice. The first was that of Catharine Bousergent, a French woman, forty years of age. The disease had existed from infancy. When single she drank three pailsful daily; but after marriage, when pregnant, only two. The quantity was stated by others to be sometimes four pailsful.

The second case was that of a man, in England, aged fifty-one. The disease had existed twenty-three years, and came on after a long continued fever and ague. The quantity he drank, amounted to sixteen or seventeen quarts daily.

The third subject, was a boy of five years; his pulse from eighty to eighty-five. His daily quantity amounted to about ten quarts.

OBSERVATIONS ON THE PHOSPHATE OF IRON.

BY JAMES THACHER, M. D.

[For the New-England Journal of Medicine and Surgery.]

When a new remedy is introduced for the cure of diseases, it becomes a subject of public investigation, and naturally excites among the medical faculty, a degree of interest and speculative attention, in proportion to the character it sustains in the view of its advocates. The scientific investigator will enlarge its chemical principles, and carefully examine its medical properties, the ascertainment of which is requisite to enable the prescriber to apply the medicine to particular diseases, and to accommodate it to the different constitutions and various attending circumstances. Without such previous knowledge, we are perpetually liable to defeat our own intentions by any combination; as even the vehicle in which the medicine is administered, may decompose its principles, and render it inert, or perhaps injurious. The application of these remarks, is intended for the phosphate of iron recently presented to the public, through the medium of the New-England Medical Journal, by Dr. Caleb Miller. Phosphorus, although not generally employed, is known to possess the most active properties, requiring the utmost caution in its administration, as it may on some occasions produce deleterious effects. When administered, says Dr. Willich, (Domestic Encyclopedia,) with judgment and circumspection, phosphorus forms a very powerful and valuable remedy. It has proved of essential service in paralytic, epileptic, melancholic, and maniacal attacks; in every species of eruptive fevers, particularly in those where the eruption *strikes in*, and is accompanied with anxiety, convulsions, and other nervous symptoms. M. Leroi, in France, administered phosphorus internally with the greatest success, in consumptive diseases; in malignant fevers where it effectually checked the progress of gangrene, and especially contributed to the recovery of persons who had been reduced by intemperance. The dose is not to exceed one or two grains in twenty-four hours, exhibited in some mucilage to prevent its too violent action on the coats of the stomach. The acid produces similar effects in the system with the solid phosphorus. It is obviously important, therefore, that whatever preparation of this potent medicine be employed, the prescriber possess accurate knowledge of its chemical relations, and of the precise proportions which the component parts bear to each other. The phosphate of iron may be obtained by mixing solutions of the sulphate of iron, and the phosphate of soda; but this is not the preparation of Dr. Miller; his process is reserved as a secret in his own hands. Every one is aware that it is inconsistent with the liberal principles of medical science, to countenance or support the employment of secret compositions; and a physician who is actuated by the genuine principles of honour and rectitude, will hesitate, before he subjects his patients at random, to experimental operations, for the ascertainment of the effects of new remedies with whose properties he is altogether unacquainted. But in those cases where there is no risk of dangerous consequences, and the patient suffers no pecuniary inconvenience from the practice, it cannot be deemed reprehensible; and on some occasions the most beneficial effects may be anticipated, and the knowledge of practical medicine thereby essentially promoted.

I was indebted to Dr. Miller for a sample of his preparation for trial, at the moment when I was under considerable embarrassment with an obstinate case of amenorrhæa of more than two years continuance. The patient was a woman in connubial life, who, having been the subject of laborious parturition and instrumental delivery, experienced no natural recurrence of menstruation. After a lapse of one year in feeble health, she was attacked with severe periodical pains, indicative of the efforts of nature to restore the uterine function. The usual course of remedies was now adopted, and steadily persisted in for several months without effect. At length, the obstruction yielded for once to bloodletting and the continued use of the tincture of black hellebore; but having again recurred, the common routine was repeated, and occasionally varied, without success, and she was afflicted with severe pain from one to two, or even three weeks during each month. She commenced the use of the phosphas ferri, in doses of about half a drachm three times a day, and had taken to the extent of six ounces without producing any perceptible effect on the system, except that at first it excited nausea and vomiting. Apprehending a disappointment, I resolved to attempt facilitating the desired effect by the employment of the lancet; this happily succeeded, and in a few days she experienced a restoration of the menstrual evacuation, and has since, being about four months, suffered no interruption to its natural periodical course.

My next subject of trial was a single woman, who, by imprudent exposure, contracted a cold during the critical period, by which a suppression immediately ensued. She was soon attacked with fever, accompained with a train of distressing symptoms, the most prominent of which were flatulency, indigestion, a distressing sensation in her throat, and difficult respiration. Her mind was constantly harassed with various imaginary evils, and the terrors of death were continually painted to her view. After premising the proper evacuations, I prescribed half a drachm of the phosphas ferri every four hours; the beneficial effects of the medicine were soon evinced by the abatement of the symptoms in general, but particularly by the improved condition of the stomach and digestive powers. Having persevered in the use of it about eight or ten days, she was so far recovered, as to dispense with my attendance, and the medicine was discontinued. The patient progressed rapidly to the standard of perfect health, though it was several weeks before menstruation was restored to its wonted course.

A young woman had suffered a suppression about four months; the symptoms were, defective appetite and difficult digestion, great debility, pale countenance and emaciation. The phosphate of iron was directed three times a day, and aloetic pills occasionally, by which, at the expiration of six weeks, the amenorrhæa, with all the appendant symptoms were completely removed. Another young woman, in whom the menstruation had been suspended about five months, exhibited the marks of incipient hectic fever when I was first called to visit her. Regular exacerbations of fever, occurred twice in twenty-four hours, and she had profuse night sweats. These were accompanied with a distressing cough, and most copious expectoration; her appetite and digestion were much impaired, and her system was in a state of general debility and rapid emaciation. Under these alarming circumstances, I adopted the plan as practised by the late Dr. Senter, in phthisis pulmonalis, and accordingly prescribed the dry vomit of sulphas cupri and ipecacuanha every third morning, and the myrrh and steel mixture of Dr. Griffith three times a day. By a perseverance in this course for four weeks. I had the satisfaction to observe that very essential advantages were experienced; the cough, the expectoration and night sweats were greatly diminished, and the hectic symptoms had subsided. A restoration of the menstrual evacuation, was now deemed a desirable object; but from the great debility of the patient it was scarcely conceived to be attainable. Upon the principle of its tonic and restorative powers, I resolved to employ the phosphate of iron, discontinuing the other remedies, I directed a large teaspoonful of the powder three times in a day. During the first week of its use, I was assured that her stomach was much invigorated, and the patient and her friends were encouraged by the visible good effects which they had conceived the medicine had produced. But at the expiration of four weeks, having taken twelve ounces of the medicine without deriving any essential advantage, and as it appeared to induce a costive state of the bowels, I directed it to be dispensed with.

A female patient, aged fifty-seven, has for several years labored under chronic debility, and is miserably afflicted by a complication of complaints, such as diminished appetite, indigestion, extreme anxiety and dejection, flatulency, and troublesome borboragmi. Wearied with the ineffectual application of remedies of various description, she at length resorted to opium as the only medicine that could palliate her sufferings. Her system being continually under the influence of the stimulant effects of that drug, a fair opportunity could not be afforded for the legitimate effect of any other medicine; the phosphate of iron, however, was administered, although no decisive result was to be anticipated. Either from the novelty, or the actual power of the medicine, she conceived so favourable an opinion of its invigorating effects, and its tendency to obviate her flatulent complaints, as to induce her to apply to me for repeated supplies, and she still perseveres in its use, and has expended about ten ounces.

Another woman, about thirty years of age, has been for several months greatly affected with dyspeptic complaints, accompanied with distressing sensation of heat and burning about the region of her stomach, and extending to various parts of her body, languid circulations, extreme coldness, and numbness of the extremities, and general debility. Being of a capricious disposition, her stomach had been improperly gorged with a variety of articles of the materia medica, and her complaints were in no degree diminished. My advice being requested, I should have prescribed the oxide of bismuth; but not having that medicine on hand, the phosphate of iron was substituted. This soon appeared to have a favourable effect on the morbid state of her stomach, and evidently afforded considerable relief. A perseverance in the use of the medicine was enjoined, and in two or three weeks its efficacy was more decisively demonstrated than any other remedy which she had employed, and a daily amendment was observable. She has recovered the use of her limbs so far as to walk out for the benefit of the air; but the painful sensation of heat is not much diminished; and although she weaned her child about nine or ten weeks since, the catamenia have not yet reappeared.

The phosphate of iron has, in most of the cases where I have prescribed it, appeared to exert its action on the stomach, and improve the powers of digestion. It has in every instance imparted a dark colour to the fœcal discharges, simiiar to that produced by the employment of chalybeates. It has in no instance occasioned any deleterious effects, however amply employed, except an overdose seldom fails to excite nausea and vomiting. I am not satisfied that it has manifested a specific action on the uterus; but when menstruation has been promoted, it appeared to be the effect of an improved condition of the stomach, and a restoration of the integrity of the general system.

Upon the whole, I have conceived an opinion, that a combination of phosphorus and iron, when the most eligible preparation can be ascertained and appropriately applied, will be found a valuable addition to our pharmaceutic remedies.

Plymouth, June 1st, 1815.

CASE OF FATAL COLIC FROM THE LODGMENT OF A CHOCOLATE NUT IN THE APPENDICULA VERMIFORMIS.

BY OLIVER PRESCOTT, A. M. *of Newburyport.*

[Communicated for the New-England Journal of Medicine, &c.]

As I do not recollect meeting with any case on record similar to the following, and as it is very possible that an extraneous substance, such as plum or cherry stones, may have in other instances given rise to similar cases, and thus have proved the hidden and unsuspected cause of obstinate and incurable colic; I have thought it might be useful to make a communication of it to the public.

On the afternoon of Tuesday the 10th of January, 1815, I was requested to visit captain Parker Robert of this town, aged forty-two years; he has for some years past been master of a merchant vessel in the European trade; his health has always been good, never having experienced any sickness or considerable indisposition in his life, or had occasion (as he says) to apply for medical aid, excepting in one instance, which was at Cadiz about twelve months since, as will be mentioned. His complaint was an obtuse, deep seated pain in the right side of the abdomen, just above the os ilium, but towards the lumbar region, accompanied by a tenderness of the bowels generally. He has experienced these sensations constantly, more or less, for several days; and expecting that he should find relief by an evacuation of his bowels, he had taken of his own accord, two days previous to my seeing him, a dose of rhubarb, which he says operated freely, but did not afford the expected relief, or produce any alleviation of the pain. His eyes had a yellow tinge, and his countenance exhibited a jaundiced hue, but there was no hardness or acceleration of his pulse, or morbid heat of the surface; his tongue also was clean. From the part pointed out, as the more immediate seat of pain, I concluded its source to be in the cœcum or commencement of the colon, occasioned by a deficiency of bile in the intestines, and probably an accumulation of indurated fœces.

I prescribed a few grains of calomel, and directed them to be followed in half an hour by a dose of pills composed of aloes g. gamb. and nitre. When I put the pills into his hand, he enquired if they were bilious pills, saying that he had once experienced a similar affection of the bowels, about a year since, when at Cadiz; that he applied at that time to an English physician for advice, who gave him for the complaint a few pills, which he called bilious pills—these operated well, and afforded so much relief that no other medicine was taken, but that several days elapsed before his bowels felt entirely free from tenderness.

He took but four pills, which produced four copious ejections, and each operation was followed by mitigation of his pain. About the middle of the following night, he complained of feeling cold, and had a severe ague fit, his wife applied a warm woollen blanket next to his body, and passed the warming pan about him. As the chill went off, the pain returned with increased violence, and soon became so severe that I was sent for. I found him in very great distress, the pain being fixed altogether in the place of its origin, viz. above the spine of the right ilium, and as I supposed at the commencement of the colon. His pulse was small, but slow and soft; and as the cathartic had operated so freely, the pain was now attributed to spasm. A dose of opium was accordingly prescribed and repeated two or three times; the whole quantity given might have been eight or nine grains in all, after which he became easy and disposed to sleep; but this calm, however, was only of short duration, for in about half an hour he aroused in very great agony—but his distress was now of a different kind, his sensations being as if his bowels were distended to the utmost, so that there could be no room for him to breathe; he felt also a pressing inclination to pass his urine, and said repeatedly that his bladder would burst if his urine did not soon flow. His abdomen on examination was found rather tense, but not greatly swelled or inflated, and there was no such tumour above the pubes as is usual where much urine is collected in the *vesica urinaria.* Dr. Bradstreet was called in at this time, and concluded with me in consultation, that these symptoms were occasioned altogether by spasm. Camphorated oil was rubbed on the abdomen, a large epispastic applied over the original seat of pain, and an enema administered, which was also soon repeated. In an hour or two he found considerable relief, but without having passed any urine; during this distress preparation was made for drawing of blood, but the coldness of the surface and smallness of the pulse deterred us from opening a vein. Eight grains of calomel with camphor and nitre were now prescribed, with directions to repeat the like dose every six hours; he was also directed to take frequently of supercarbonate of potass dissolved in water, in the proportion of one drachm of the former to two pounds of the latter. I visited him at evening and found he had passed a more comfortable day than was expected, and his urine had passed without much difficulty; he was directed to continue the same course of medicines, and to take two grains of opium if the pain should become severe.

12*th*, *Thursday.* He has had a night tolerably free from pain, but has taken opium at different times to the amount of six grains in all—perspiration is free and has been so through the night—the pain has shifted and is now most severe on the opposite side of the abdomen. The epispastic was directed to be applied over the part now in most pain, and the patient ordered to continue the powders and follow the other directions. In the evening Dr. Noyes visited him with me, and advised to a continuance of the same course of medicines, &c. and as there had been but two very small discharges from the bowels this day, or even since the morning of the 11th, it was agreed that he should take two ounces of ol. ricini early the next morning: his pulse is full and soft.

13*th*, *Friday*. Visited this morning with Dr. Noyes—he has taken but one grain of opium and has had considerable rest during the last night—the symptoms and his appearance are such as encourage the hope of a favourable termination of the disease—the powders of calomel, nitre and camphor, and the solution of supercarbonate of potass were advised to be continued, and he was directed to take this afternoon one ounce of manna and one fourth of an ounce of senna in infusion—the like dose to be repeated every hour, until there is a free operation from the bowels. When I visited him at evening, he had taken three doses of the manna and senna, and after trying to introduce an injection, had a pretty copious discharge from his bowels whilst I was present. I would observe that a discharge from the bowels always afforded temporary relief; he therefore was often pressing to have an enema injected. In the night he became extremely sick at the stomach, and puked several times—I was sent for, and after applying appropriate remedies the nausea wore off.

14*th*, *Saturday*. I found him at the morning visit tolerably easy, but he had had no stool since the one in the evening—I prescribed a pill containing two grains of aloes to be taken every second hour, and every other hour two grains of calomel, and that he should take two grains of extract of hyosciamus whenever the pain should prove severe. In the evening his abdomen was much less tumid and he had less pain, but there had been no alvine discharge—he was therefore directed to continue the pills, but to increase the dose of calomel to three grains—the camphor, &c. to be continued.

15*th*, *Sabbath*. I was sent for at nine o'clock A. M. He has enjoyed a comfortable night, with frequent naps of sleep, &c. but he now feels again that sensation of fulness and distention as if bursting—there has been no discharge from his bowels since the evening of the 13th, although numerous injections have been administered. He was now directed to take one drachm of manna in a solution of cream of tartar, and repeat the dose every half hour—three ounces were thus administered in the space of one hour and an half; it produced no operation, and the distressing sensation of his bowels being ready to burst asunder increased. A blister was applied over the stomach and bowels, with such other directions as were deemed most appropriate to the circumstances of the case. At two o'clock P. M. Dr. Noyes visited him with me—there has been no operation from the cathartic medicines, and injections cannot now be received or retained—his pulse is accelerated to one hundred and forty beats in a minute, and are so small as to be felt or counted but with difficulty—his extremities are cold, and his breathing very laborious—his countenance is also changed, and he is evidently dying—he lived until about nine o'clock in the evening and then expired.

Permission having been obtained, his body was opened and inspected the day after his decease, by Dr. Noyes and myself. Inflammation had universally pervaded the abdominal contents, the intestines were agglutinated together and covered with coagulated lymph, and were found adhering in many places to the peritonæum; the cœcum and a considerable piece of the colon in its vicinity, as also part of the ileum was in a complete state of sphacelation, and a small hard substance protruded through the cœcum, at the entrance of the appendix vermiformis, or rather at the mouth of the appendicula. This substance at first view was taken for a calculous concretion, but on applying the point of the scissors a piece of it broke off, and proved to be a cocoa or chocolate nut; this had been lodged in the entrance of the appendix, and no doubt was the immediate cause of the patient's death; and probably the same kind of complaint, mentioned as having happened at Cadiz a twelvemonth previous, was occasioned by this same nut. There were no scybala or hardened fœces discovered in any part of the intestines—the liver shewed no strong marks of disease or inflammation, nor was it enlarged—the gall bladder was full and much distended with bile; finding it could not be emptied by pressure, it was opened, its contents were black as ink, and of a thick consistence like tar; the stain on the hands from it could not be washed off by soap and many waters; the stain was of a deep yellow colour—the biliary ducts were found to be impervious to a probe, &c.

The family of the deceased have never heard him mention the circumstance of his having at any time swallowed such a substance: the chocolate nut must probably have passed into the stomach by some accident, and by its gravity have obtained a lodgment in this receptacle, and have filled it too completely to be removed. It might have remained in this situation for a long space of time and proved inert, whilst the bowels were properly supplied with healthy bile, and discharged their functions in a proper manner; but so soon as the bile became obstructed, the deficiency of this necessary fluid occasioned torpor and spasm of the intestinal canal; under such circmstances this extraneous substance became mischievous and productive of irritation, inflammation, gangrene, &c. Whether this reasoning be correct or otherwise, the nut thus lodged, proved unquestionably the immediate cause of our patient's death. It was too firmly wedged in the appendicle to have been displaced by any means which could safely have been employed; but had its existence in such situation been foreseen, venesection, the warm bath, and a free internal use of some bland oil might probably have afforded the most likely means for relief.

CASE OF SPASMODIC COUGH, SUCCESSFULLY TREATED WITH STRAMONIUM.

[Communicated.]

MESSRS. EDITORS,

The subject of the following case, Mrs. M. W. is of a weak irritable habit of body; the mother of several children.

October 2d, 1809, I received the following account of her complaint. Since April last she has been troubled with spasmodic cough, somewhat similar to hooping cough, and frequently terminating in a convulsive fit. To these are also joined palpitation of the heart, subsultus tendinum, periodical vomiting and an irritable and frequently distressed stomach, oppressed upon the least exciting cause. No relief of any importance has been obtained from medicine, during the above time. During constant and exclusive attention to a favourite child dangerously sick, the above complaints were much alleviated, and in a degree suspended. At the death of her child, great irritation of mind ensued, with a slight recurrence of former complaints, particularly cough, fits, and spasms at the stomach. Three weeks after the death of her child, her former complaints assumed a more formidable aspect, and threatened an annihilation of remaining strength. Vomiting returned, which was checked by wine, rendered strongly aromatic by cloves and cinnamon, a remedy suggested to her in a dream and adopted by the consent of a former physician, and which had checked the vomiting at a former period. Fits of coughing came on which commenced with slight irritation in the throat, and terminated in a convulsive fit. The cough, previous to this time, had never been preceded by repeated efforts at deglutition, but came on with a convulsive motion of the head and throat, which lasted one or two minutes, and most commonly terminated in a fit, which last continued from five to thirty minutes. During the fit the head and extremities were convulsed, and a total insensibility ensued. The fits at this period, November 18th, were frequent and distressing, attended with a noise from the throat which appeared to arise from a convulsive motion of the muscles of deglutition.

Previous to this period, digitalis, bark, and various antispasmodics were administered without affording the desired effect. It may, however, be observed that blisters were applied at a former period, without the least effect, and that opiates in various forms have been administered and have been constantly and uniformly rejected.

On Thursday, November 20th, when the cough and fits assumed a more distressing aspect, recurring eight or ten times during twenty-four hours, and threatening destruction of the powers of life, two grains of the yellow sub-sulphat of quicksilver were administered and repeated every twenty minutes until eight grains were taken, which occasioned a slight nausea and considerable distress at the stomach, attended also with a copious perspiration during the night, but no evident relief to the existing complaints.

Friday, November 21st, A. M. nine o'clock, two grains of the extract of the datura stramonium were administered; 1 o'clock, P. M. three grains; 6 o'clock, P. M. four grains; 10 o'clock, P. M. two grains through mistake. During this day three fits only, the last of which was between five and six o'clock, and continued twenty minutes, attended with convulsive motions of the head, throat, and extremities, and succeeded by great distress at the stomach, and with soreness about the throat, with difficult deglutition.

Saturday, November 22d, no fit during the night; six grains of the stramonium; 3 o'clock, P. M. no fit, cough subsided, slight glow over the body, accompanied with a dull obtuse head-ach; 6 o'clock, P. M. has experienced slight irritation in the throat once or twice to day, but no coughing or return of fits; dull head-ach.

Sunday 23d, no return of fits or cough; dull obtuse pain in the head with a sense of stupidity, pupil sensible, as has been the case during the administration of the medicine. Bowels costive; opened by injection.

December 1st. From the last mentioned date has had no return of fit or cough. The subsultus tendinum had ceased at the time the cough and fits left her. At present the chief complaint is a dull obtuse head-ach which has continued since the exhibition of the stramonium. Appetite indifferent; bowels constipated.

Memoir of an Epidemic Fever which prevailed at Wardsborough, Vermont, and its vicinity, during the summer and autumn of 1814, *together with some Observations relating to its Remote Causes and Mode of Treatment.*

BY JONATHAN A. ALLEN, M. D.

[Communicated for the New-England Journal of Medicine, &c.]

In the winter and spring preceding the epidemic, the diseases of this place were similar to those which occasionally occurred, at the time, in other parts of the country. They varied little or none from the disorders of 1812—13, which were rife in many places, especially at Greenbush and Plattsburgh, N. Y. They commonly yielded to the usual remedies.

Typhus mitior appeared in several instances. None proved fatal. Several cases put on very alarming symptoms. Hæmorrhages were common. Blood was discharged by the mouth, nose, and per anum et uterum.

Cynache parotidœa made its appearance in the spring. This and the typhus in several instances partook of the nature of the pneumonia typhoides; the former between the third and seventh, and the latter between the tenth and fourteenth days of the disease. After this the patient became convalescent in about a week.

The bloom of May was marked by a change of diseases. Typhus gravior in a few instances appeared. Three cases terminated fatally in one house. In neither of them, were any extraordinary or uncommon symptoms observed, unless their violence be esteemed such.

At the termination of the two last cases, which happened within a few hours of each other, the weather was unusually humid and warm. The corpses suddenly run into the putrefactive process, and most unfortunately as it was, they were not deposited in sufficiently close coffins. The effluvium evolved from the diseased was very offensive to the people who attended the funeral ceremonies. Here were sown the first seeds of the epidemic. Nearly all those who were exposed to those septic gases had an attack of the disease; and from the sick it was communicated to their attendants throughout the season, except instances in which much caution was used. The interim of time from exposure to an attack, was from ten to twenty-one days.

In some instances the attack was sudden; in others, the person would be lingering a number of days.

The disorder commenced with a stiffness and pain in the nape of the neck, severe pain in the head, especially over the orbits; pains wandering and acute in different parts of the body, resembling rheumatism or membranous inflammation; chills, rigours, anxiety, cold, and hot paroxysms quickly alternating; numbness and coldness of the extremities; the palpebræ swollen and tunica adnata of the eyes suffused with tears; a wild, vacant stare with the eyes, and the head always preternaturally hot; the pulse somewhat increased in frequency, and generally soft and small in the extremities, whilst in the carotid and temporal arteries there were more strength and force; in others, the pulse would be considerably full at the wrist.

The tongue was commonly covered with a yellowish coat, except in some cases, of a very disagreeable nature, in which last it was smooth, red, and dry. In these cases hæmorrhages were to be expected.

There was always nausea at the stomach, and very disagreeable bitter taste in the mouth. Pain at the scrobiculus cordis was common and sometimes in the umbilicus. The primæ viæ were frequently with difficulty excited to action, and the ejections were usually fetid and bilious. The breath was very offensive and hot, though in the first stages of the disease, there was seldom apparently any disease in the chest. The skin communicated to the hand a burning sensation, essentially different from common inflammation.

In almost every instance, the functions of the animal organs were deranged. This was characterized by impaired sensation and volition. In some instances perception from external impulses was almost obliterated; in others, the most exquisite sensibility prevailed, and occasionally these extremes would alternate. Delirium and stupor were common symptoms; either denoted a high degree of diseased excitement, but did not always portend death.

In more mild cases, the susceptibility of the animal organs to surrounding agents, apparently continued as in health; but the intellectual operation made no durable impression, consequently on recovery the patient would have but a faint, or rather no recollection of the various scenes which were passed during his illness.

The incipient stage was frequently attended with an erysipelatous inflammation. This, in favourable cases, in a few days subsided; in others, it turned purple, or of a dark colour.

As the summer season advanced, bilious symptoms became more predominant. The typhoid nature of the complaint increased. A sinking diarrhæa more generally ushered in the disease, which continued more or less troublesome during the disorder. The skin assumed a more sallow hue; but yet the disease retained the distinctive characteristics of the epidemic at its commencement. The distress in the epigastrium and umbilicus increased. The alvine evacuations varied from green to yellow, and to a chocolate appearance.

Proportionally as the autumnal equinox approached, the disorder changed to a more mild species, but still retained the original marks of its character.

During the season the remissions were imperfect; the paroxysms were irregular, both in respect to accession and duration. They commonly, however, continued from three to twelve hours.

An exacerbation most generally happened every afternoon, but frequently in the morning or at midnight, and even there, in the same patient, were alike in nothing, but irregularity of accession. If there were any regularity, it was in a greater number of exacerbations happening every afternoon; hence shewing some small resemblance to the tertian type.

The usual duration of the disorder was from seven to twenty-one days. No critical evacuation or crisis could be observed. An amendment began, by an abatement of the severity of the exacerbation. This always commenced when the system could be made to be properly affected by medicine. If no subsidence of the symptoms was to be discovered by the third diurnal septenary, the patient died; I have known of no exception.

In the last stages of the disease, petechiæ were common appearances. If the petechiæ from a fiery red changed to a purple, the change denoted a fatal termination. In favourable cases they ere long receded. A peculiar sallow countenance, in all cases, was very perceptible. This was so evident as to be observed by the people, and considered by them as a criterion of the *fever*. This appearance varied from a slight discolouration to a chocolate colour.

The symptoms denoting dissolution were Hippocratic face, furious delirium, or death-like stupor. Death was often ushered in by a gradual declension of the vital powers; in some instances, however, the agonizing convulsions and painful efforts of life were shockingly distressful.

The plan of treatment which proved most successful, was somewhat various, yet in many respects always similar.

If the inflammatory diathesis admitted it at the first onset, venesection was practised; this was of decided advantage in many instances. The blood was usually drawn through a large orifice, and the quantity taken was proportioned to the effect made on the pulse. This operation was performed at all stages of the complaint, whenever the excitement seemed sufficiently increased to admit it. In my own case it was practised on the eighth day with real benefit. In those cases of an asthenic nature, resort to it was not had.

In order then more effectually to interrupt the catenation of diseased actions and to divert the system of noxious principles, emetics and cathartics were used. These sometimes were administered separately, and at others combined. At the commencement of the epidemic, tartrite of antimony and submuriate of mercury were given united. The operation of this *emetico-cathartic* always produced a temporary mitigation of the symptoms, and in many instances, the morbid associations were immediately dissevered, and health quickly restored.

In less violent cases the emetic preceded the cathartic. The former was commonly tart. of antimony and ipecacuanha, and the latter pulv. rhei. or jalapæ and submuriate of mercury. Large cathartics, in some instances, required assistance from enemas.

When, in other instances, the commencement was marked by a sinking diarrhæa, emesis was promoted by the use of sulphate of copper or tin united with ipecacuanha, or what I conceive equally efficacious and more palatable, a solution of either of the former acidulated with sulphuric acid. These emetics during the continuance of the complaint were repeated at various times, according to circumstances. In the first stages, as well as during the whole disorder, every exertion was used to equalize the excitement by an assiduous application of refrigerants to the head, and stimulants to the extremities. To accomplish this intention, recourse was had to cold water, vesicatories, stimulant cataplasms, &c.

Cold affusion in all cases was made freely to the head; and whenever the excitement became sufficiently general, was used with material benefit as directed by Drs. Currie and Jackson. Ablution and aspersion were also used, but affusion seemed most serviceable, on account of its making a more sudden impression.

Ventilation and cleanliness were among the most important objects of consideration. The windows and doors were thrown open, and the linen changed every day. All excrementitious substances were immediately removed, and all effluvia properly corrected. In no instance have I seen ventilation and cleanliness injurious; but, on the contrary, of the most decided advantage when properly regulated. The patient was not only for a time refreshed and relieved from the scorching flames of a burning fever, but the nurses freed from the destructive consequences of breathing deleterious and pestilential gases, which seldom or never failed of producing the disease in persons debilitated from whatever cause, as fatigue, anxiety, &c.

Here it may not be improper to notice the deadly consequences of the pernicious practice of one of those mean animals called *Quacks*. In detailing this, I cannot do better than to quote the words of Dr. Laban Hazeltine, an ingenious and worthy young man, as they were used in an Inaugural Dissertation, read before The Second Medical Society in the State of Vermont:

"This man," says he, "got it into his head somehow or other, that fever is a *size* in the blood, and, to use his own language, it must be *sweat out*, or the person must die. Hence he used hot drinks, as rum, brandy, and the like; kept his patient under an enormous load of bed-clothes; kept all air from his apartment for fear of *striking in the size*, as he called it. Hence every crevice was stopped from which any air might enter to cheer the unhappy sufferer. Here then his patient lay, oppressed with a hot disease, in a hot room, in a hot season of the year. What could be expected from such practice, but death to the unhappy sufferer, and the seeds of death to his neighbours."

It was so. Pestilence did not walk in darkness, but was nourished at noon-day, till fatality taught, by sad experience, that cunning speeches, grave looks, self-conceit, and vain sanctity of appearance, would not supply a deficiency of honesty, medical information and skill; and the people discovered the imposition.

When the disease was not subdued by the first impressions, recourse was immediately had to mercury. If a moderate and friendly mercurial action could be produced, a favourable prognosis could be given with almost certainty. This mercurial action was greatly favoured by keeping, when possible, a moderate diaphoresis with some sudorific powder.

Cathartics, during the complaint, were administered agreeably to the directions of Dr. Hamilton, in typhus fever; for this purpose calomel given only once or twice, in twenty-four hours, in sufficient quantity to produce the double effect of a gentle cathartic and ptyalism were most serviceable.

All the mineral acids were used, especially the muriatic, as recommended by Dr. Thomas in typhus gravior. These were of manifest service.

The alkalies in some cases were alternated with the acids, and particularly when putrescent symptoms intervened. The effervescing mixture was much used.

Fermenting beers afforded an excellent and salutary beverage, and were taken in all cases.

Enemas were frequently administered, more particularly those which would afford an evolution of carbonic acid gas.

In all obstinate diarrhæas, the acetite of lead, from three to six grains combined with opium, was given every three or four hours till the intention of its exhibition was accomplished. From the success attending its use, and the united testimonies of Drs. Archer, Ewell, and Professor Mussey, I am induced to believe it to be not only a harmless, but, in such cases, one of the best of remedies.

In low cases the more effectually to cleanse the skin, ablution was freely made with *soap suds.* To rouse the vital flame, capsicum and tincture of cantharides were exhibited; the latter, more particularly, seemed to renovate the sinking spirits, calm delirium and diminish stupor.

Either when the fever subsided or the living powers seemed failing, bark, wine, columbo. &c. were truly useful.

Epispastics, in all stages, proved highly serviceable, especially during stupor or delirium.

Under a judicious application of the preceding means, not more than one in sixty failed of being restored to the possession of health, in a degree superior to what they usually enjoyed.

The success attending the contrary mode of practice needs no more detail.

READINGS AND OBSERVATIONS—No. III.

TINCT. MURIATIS FERRI.

In the Medical Records and Researches selected from the papers of a private Medical Association of London, we have the detail of a case in which the above article was used with success. It was one of those suppressions of urine which arise from a spasmodic affection of the urethra. The subject of this case had a stricture in the urethra during several years, for which bougies were occasionally used. From the frequency of the attacks, this man came to London to be under the care of Mr. Cline, who for a very severe attack ordered a tobacco injection which relieved him; another attack soon occurring, the injection was again prescribed, but the distressing symptoms which attended its first use rendered the patient extremely averse from its repetition, and it was proposed to give him ten drops of the tinct. muriatis ferri every ten minutes, until it produced some sensible effect. When he had taken six doses of it, i. e. in an hour, he had an inclination to void his urine, and it immediately flowed freely; although in every preceding attack the complaint had continued about forty-eight hours, except when the tobacco clyster was given. The same medicine in similar doses always relieved the attacks he afterwards suffered. "This medinine has since been given in several similar cases with equal success; but in all, the complaints were purely spasmodic. Retentions of urine from other causes cannot be affected by it." Nausea and vomiting are among the effects of this medicine, and it is observed in another work, that nausea is the precursor of its antispasmodic effects in the cases under consideration. It has been tried here in similar cases, and in chronic ones in broken constitutions with decided benefit.

TIC DOLOUREUX.

In a case of Tic Doloureux in which cicuta was given very freely, and without the least apparent benefit, (which fact, however, from the testimony in favour of this article contained in a former number of this work, led the practitioner to believe it had been injured by exposure and time,) the alcohol ammoniatum was used with great benefit. This article was given in doses commencing with thirty-five drops three times a day, increasing one drop a dose. The remedy was continued till nearly a drachm was taken three times daily, when the disease gradually disappeared. The above was a very severe case of this painful disease.

BLISTERS.

Blisters have acquired of late a good deal of reputation from the beneficial effects which have followed their application to suppurated tumours, before their contents have been evacuated. But they may be found useful in large abscesses which have broken, and from whose surfaces large and debilitating purulent secretions are constantly made. The details of a case now lay before me, which contain some particulars of a deep and large sinus in the neighbourhood of the rectum, from which a large purulent discharge was constantly kept up. In two or three weeks another tumour was observed to be forming near the trochanter. It became necessary in consequence of the weakness which attended the secretions from the first ulcer to prevent if possible any evacuations from this. The usual means being tried in vain, a blister was applied on the part where fluctuation was most distinct and extending to the neighbourhood of the sinus above mentioned. Absorption rapidly took place in the tumour and it disappeared; the discharge from the sinus diminished and soon ceased, and the patient recovered perfect health. In this same case, obstinate retention of urine occurred, for two days under a variety of treatment this distressing affection continued. Tincture of cantharides was at length given in doses of fifteen drops with very speedy and continued relief.

TINCTURA CANTHARIDUM.

Dr. Home in his clinical histories and experiments informs us, that on the authority of Hillary, who wrote on the diseases of Barbadoes, he was induced to give the above mentioned article a fair trial in typhus fever. He gave twenty drops of the tincture in some wine whey twice a day. From a number of experiments he infers that it is a very innocent remedy. By combining it with mucilage he was enabled to give a third more of it than Hillary. Thus he gave thirty-five drops four times a day in cutaneous diseases, and fifty, four times a day in diabetes, without any sensible effects on the urinary passages or elsewhere, except some gripes and slight strangury. In four cases of typhus out of five mentioned by Dr. Home, the tincture above mentioned appeared to be of service.

RULES TO BE OBSERVED IN NOTING THE HISTORY OF A DISEASE.

Pinel Med. Clinique—p. 5.

In order to make an exact record of the history of a disease there are two objects of attention; the one relates to the circumstances to be learnt when the patient first comes under one's care; the other is to note from day to day the course and progress of the disease.

First. The researches to be made at the first visit may be ranged under five principal heads.

A. The description of the actual situation of the patient, by noting, 1st, those symptoms which are obvious to the senses: 2d, the pains which the patient experiences: 3d, the successive analysis of the state of the several functions.

B. To go back to the origin of the disease in order to compare the present with the preceding state. For this purpose one should become acquainted, 1st, with the particular character of the invasion: 2d, with the symptoms which have occurred since the invasion: 3d, with the period of the occurrences of the symptoms now existing.

C. To seek the exciting and predisposing causes: these will be found, 1st, in the profession and manner of living of the patient: 2d, in the state of health, and the particular accidents preceding the present disease: 3d, sometimes in the diseases to which the parents or family of the patient have been subject.

D. These researches will suffice to make us acquainted with the species of the disease, unless it is not yet sufficiently developed. The species being determined fixes the general principles of treatment.

E. We must now attend to the varieties oocasioned by the particular circumstances of the patient, such as age, sex, temperament, and constitution. We must calculate the changes which these circumstances may make in the symptoms. Lastly, we must determine the modifications of the general principles of treatment, in order to adapt them to the particular case under care.

Second. To render an account day by day of the course of the disease, less remains to be done; 1st, we should note whether the principal symptoms have increased, diminish, or remain the same: 2d, we should remark any new symptoms which show themselves: 3d, we should notice the state of the secretions, as it relates to the three periods of the disease: 4th, we should observe the effect of remedies: 5th, fix at the decline the epoch of convalescence, and determine the rules of regimen.

HAZELTINE ON SPOTTED FEVER.

MESSRS. EDITORS—GENTLEMEN,

If it should not be convenient to re-publish in your Journal the whole of the valuable paper, which appeared in the Portsmouth Oracle, on the disease called "Spotted Fever," as it lately appeared in the county of York, Massachusetts, by Richard Hazeltine, M. D. the following brief abstract, consisting almost entirely of quotations from that paper, and relating principally to the history and character of the disease, will be interesting.

A FRIEND TO THE AUTHOR.

The learned author considers the disease a species of typhus, and the same as has been of late years so prevalent under the name of spotted fever. It did not to his knowledge make its appearance in the county of York till 1810. In 1811 and 1812 many cases occurred. Children were principally its victims. It then disappeared till the autumn of 1814, since which it has continued with undiminished violence till the spring of 1815. The disease in this last visitation has not been so suddenly mortal as in many other instances on record. The most frequent symptoms of adults were rigours succeeded with preternatural heat; irregular chills and heat; great thirst in some, moderate in others; anorexia; pain at the stomach; nausea in many cases; vomiting in a few; great distress; jactitation; great confusion of thoughts, even to delirium in many cases; coma in a few; deep suffusion of countenance; pain in different parts of the head, mostly in the temples and over the eyes, sometimes, almost intolerably severe, threatening distraction; sense of numbness in the head, as the sick expressed themselves; vertigo; numbness, prickling, and other morbid sensations in various parts of the body, but particularly in the limbs; erratic pains in the body and limbs; white, moist, furred tongue, in a few instances, dark brown as if tinged with ink, dry in the middle, moist on the sides; eyes heavy and dull; sometimes brilliant and of a natural appearance, although other circumstances were very distressing; bowels regular, although there were many cases, in which the disease attacked in the form of colic; pulse weak, small, irregular, and often intermitting, and increased in frequency; great prostration of strength; and a propensity to sweat. The pulse was sometimes full, regular, and not greatly increased in frequency; the pains besides being erratic, were often spasmodic, like the cramp; there were in some cases great rigidity, immobility and soreness of the limbs and surface; spastic rigidity of the muscles of the lower jaw, and the posterior muscles of the neck; the excretion of urine sometimes increased, but often defective, and the urine itself of a dark colour and turbid; soreness of the mouth and throat independent of any effect from medicine. In some persons the disease appeared as a trifling, transient indisposition of only one or two days continuance, while in others its attack was sudden, distressing and alarming, and continued for the space of a week or a fortnight under the form of a mild typhus or synochus. In some it would attack with circumstances of great distress, which under proper treatment would almost wholly subside in twenty-four or forty-eight hours, while in others it would commence with symptoms apparently much less alarming, but would carry the patient off within the same space of time.

The most constant and striking characteristic, by which it differed from all other diseases, and by which it was immediately recognized by those somewhat acquainted with it, was its singular anomaly; it differed from all other diseases, and yet it assumed in different subjects the livery of all; yet amidst all this diversity of character, the sagacious pathologist would descry a striking uniformity, and that all the circumstances of the disease in all its forms pointed to the same remote, predisposing and exciting causes. In all the fatal cases, the body became remarkably spotted with petechiæ and vibices, both before and after death, and a rapid dissolution succeeded.

The author assigns as the remote causes of this disease, 1st, the disuse of common salt, a great scarcity of which article had for some time prevailed.

2d, Bad grain. In consequence of the prevalence of an uncommon degree of humidity and coldness during the seasons of vegetation and harvest for two or three of the preceding years, the fruits of the earth were crude and immature. In 1812 the crop of Indian corn in the county of York was hardly worth preserving. In January, 1814, the author collected from three bushels of wheat nearly a quart of ergot; and this was a fair specimen of the grain of that season.

3d, An excessive use of ardent spirits, and also in other cases a too sparing use of them by those, to whom they had become necessary.

4th, The disuse of tea, coffee, sugar, &c. which from habit had become necessaries of life, but of which the inhabitants had been almost entirely deprived during the restrictions on commerce and the war.

5th, An insufficient quantity of animal food. Many families during these hard times were obliged to live almost entirely on vegetable food, and that of an inferior quality.

6th, A long continuance of cold and damp weather aided by deficient clothing.

The exciting causes appeared to be any considerable deviation from the ordinary habits of life, such as change in diet, loss of sleep, and exposure to cold and wet. But there is good reason to believe that the disease originated in many instances from a fomes, arising from the persons and clothes of some militia stationed for a few weeks at Kittery point. The disease appearing among these men, they were dismissed*; and the disease next appeared in those families to which they returned. This fact is by no means considered as a proof that the disease was contagious, but only as an example of idiomiasmatic exhalation producing disease in persons previously predisposed.

In the treatment the writer made it his first endeavour to remove inflammation of the the brain, which he conceived to exist. His means to effect this were principally blistering and sweating. A blister was applied to the nape of the neck, and, if necessary, to the extremities and other parts of the surface. The extent and continuance of vesication was proportioned to the degree of affection of the brain. To divert the circulation of the fluids to the surface and to produce a diaphoresis, he directed pediluvia and sinapisms to the feet; the application of hot bodies, such as bricks and stupes, &c. to the surface of the body covered with the bed clothes; sudorific medicines were at the same time internally administered. During this process the head was kept constantly wet with cold water or vinegar. The author afterwards at the suggestion of Dr. Jonathan Bailey, of Kensington, N. H. made use of emetics at the commencement of the disease with the most pleasing effects. He learnt also, from that gentleman, the great utility of friction with flannel, dry or impregnated with infusions of cantharides or bell pepper in vinegar or spirit, on the extremities and other parts of the surface, whenever there was any morbid sensation, paleness or coldness. "Under this course, simple as it is, my patients," says he, "would almost without exception be brought in a few hours from a condition truly distressing and often alarming, to a state of comparative ease, composure and tranquility."

With respect to the number of fatal cases, he observes, I am not at present prepared to estimate their proportion to the number of the sick, but would just observe, that it was evidently more severe and fatal with children than with adults; and that wherever judicious medical advice was seasonably given and duly executed, a recovery was very nearly as certain as in almost any epidemical complaint, with which the people have ever been visited in the same degree.

COLLECTIONS IN MORBID ANATOMY, No. 5.

Two Cases of Aneurism of the Aorta.

CASE 1st.

The subject of the following report,died in consequence of a hæmorrhage from the lungs. The blood was poured out with so much rapidity that, according to the report of the by-standers, death took place in five minutes from the commencement of the hæmorrhage.

When the thorax was laid open, no unnatural appearance presented, except the lungs had generally a very dark hue. The right lung was sound, but the left exhibited a rupture of its substance, and had formed adhesions with the upper extremity of the pericardium, and with the large blood vessels. This cavity was defined and smooth at its brim, but its parietes were irregular. A perfectly free communication existed between it and the air-cells of the lung, so that blood could easily be squeezed from the latter to the former. It formed a bed for a roundish tumour, which was discovered by lifting the superior and anterior edge of the lung, and which had undoubtedly been the cause of the cavity. This tumour proved to be an aneurism of the aorta. It presented three distinct elevations, which were the boundaries of three sacculi, which communicated with each other and with the aorta by one common opening. That sacculus, which projected deeply into the cavity of the lung was ruptured, and through this breach the blood from the aorta had free access to the air-cells of the lung, and from thence to the mouth. The contiguous sacculus had also an opening through its coats, but it was so covered with coagulated lymph, that no blood could escape through it.

The aneurismal tumour was situated on the left side of the aorta, and was about the size of two pullet eggs. It commenced just below the root of the left carotid; and the diameter of its mouth in the aorta was about an inch and a quarter. Two thirds of this opening was bounded by a firm and smooth ring; and the mouth of each sacculus was defined in the same way. The sacculi were partly filled with coagulated lymph, which was firm and placed in concentric layers, as is common in these tumours.

Besides this large sack on the left, the coats of the artery on the superior curvature of its arch, and on the right side were diseased and dilated, and a strip only of about half an inch in width on the inferior curvature remained of the usual thickness. The extent of the arch diseased, would measure in a sound artery about an inch, but in this it measured about two inches and a half in length. The left subclavian was removed at least an inch from the left carotid, and was included in the diseased portion. The coats of the dilated portion were unnaturally thin, and between the thumb and finger felt irregular or knobby. This last circumstance was evidently the effect of the destruction of the middle coat, small particles only of which remaining, permitted the other two coats to come in contact with each other. These did not appear to be diseased; the inner coat retained its natural polish. The coats of the artery in the vicinity of the dilated portion, exhibited spots of a chalky whiteness, and the intermediate spaces were of a dark red colour. The inner coat was raised, and this spotted appearance existed in both coats.

The heart was loaded with fat, and its muscular fibres remarkably pale and soft. It was not otherwise diseased, but it had a singular malconformation; a sack appeared on the left ventricle, elevated about an inch above the surface of the heart. When laid open it was found to extend deep into the muscular walls of the ventricle, and to communicate with it by a ring-like opening, similar to the mouth of the auricle. But this opening was closed on the inside by one of the fleshy pillars, except some small holes through which a probe could pass. Its coats were thin and smooth on the inside. It was empty, as was the ventricle.

The patient was a female aged about thirty. She had been indisposed for many months, but had not received medical advice till a fortnight before her death. She complained of debility, loss of appetite, and other symptoms of dyspepsia. She had a slight cough without expectoration, and felt an uneasy sensation, oppression, or distress, which did not amount to pain, in the left side just above the left breast. She had sometimes hot flushes and night sweats, pulse generally frequent, small and weak, but not irregular. She seemed to mend at first; but about four days before her death, she had, towards morning, after a quiet night, a hæmorrhage from the lungs, for the first time in her life, that she recollected. Its continuance was short, and she was left much as before. She kept herself still, and when the fatal hæmorrhage took place, she was awaked out of a sound sleep.

CASE 2d.

The subject was a middle aged man. He was not subjected to my inspection while living; but I learned that it had been supposed that he had an aneurism of the left carotid artery commencing at its origin, and that he died in consequence of this disease. A tumour of the size of half an hen's egg presented, and was easily felt through the integuments just above the clavicle, and opposite the articulation of this bone with the sternum. The integuments being removed, this was found to be only the apex of a large tumour, which lay deep in the thorax. It was an aneurism of the aorta, and resembled a large pouch attached to the arch of that vessel by a narrow neck. The sternum and the extremities of the first ribs made a part of its anterior, while its posterior parietes were partly made up of the trachea, the included rings of which were as bare, as if they had been carefully dissected. The coats of the sack were of a firm texture, and shewed no disposition to give way. They were uneven on the inside, except a small portion connected with the aorta, where the inner coat of that vessel was continued into the sack, and retained its natural smoothness; in no other place could that coat be traced. The sack was filled with concentric layers of coagulated lymph of a whitish colour and firm consistence, except a small canal by which the arteria innominata communicated with the aorta. That vessel arose from the sack at the distance of an inch and a half from the aorta; this space was covered by the portion of inner coat before mentioned, in width nearly an inch. The left carotid arose from the sack, distance from the arteria innominata about two inches; it was impervious; its caliber had contracted to the size of a common probe, and this was filled with coagulum so firm as absolutely to prevent the entrance of the probe. The left subclavian arose from the aorta at the inferior edge of its opening into the sack. This opening was at the superior curvature of the arch; circular and bounded by a firm and prominent ring, except about an inch of its circumference, where its coats were evidently continued into the parietes of the sack. Its diameter, an inch and a half, the same as the distance of the arteria innominata from the aorta. The artery between the heart and its opening into the sack was larger than natural; but it exhibited no marks of disease. Below the rupture, if it had changed at all, its caliber had contracted.

The first seems to be a well marked case of aneurism from a dilatation of the coats of the artery. But the appearances in the other case strongly impress the mind, that a rupture of the middle and inner coats took place, at least very soon after the commencement of any disease in these coats. That portion of the parieties of the aneurismal sack between the arteria innominata and the aorta had the appearance, as if it had been neatly separated from its place in the vessel at one end, and turned back. In no other part of the sack was there the least trace of the inner coat of the artery. There was also a very exact correspondence between the length of this piece of the inner coat, and the diameter of the orifice in the aorta.

The latter case would seem to be favourable to the doctrine of Professor Scarpa, that all aneurisms are the effects of a wound or rupture of the inner coat of an artery. But the first case is decidedly against this doctrine.

WILLIAM GAMAGE, Jr.

June, 1815.

Case of Malconformation of the Heart.

The subject was a female child, born at the full time of white parents, and apparently healthy. But instead of becoming white in a few days, as infants usually do, the skin remained of a purplish hue, particularly at the extremities of the fingers and toes. The lips and gums also were of a dark purplish colour. The child, however, nursed at the breast and increased in size, and for about two months showed no symptoms of deranged organization, except uncommon feebleness, an indisposition to motion, and this discoloration of the skin. But in a short time it became subject to frequent paroxysms of violent struggling, in which the respiration was hurried and difficult; the face became black, the whole skin was injected with dark coloured blood, and death seemed inevitable; but it would recover from the fit and immediately fall asleep. The paroxysms increased in frequency; it moaned continually, but never cried; the languor increased, it slept, except it awoke to suffer. Every motion was weak; every function ill performed. The right arm was paralyzed; the eyes had a glassy appearance; aphthæ covered the mouth and spread extensively on the nates; a constant and distressing cough was added to the catalogue. It had become insensible to every thing but pain, when death terminated its sufferings, about fifteen weeks after its birth.

The body was submitted to examination When the thorax was opened, instead of the lungs, an ill shapen mass presented; it was the right auricle of the heart, covered only by the pericardium, distended to twice its natural size, and filled with black and fluid blood. The whole heart was distended with blood of this character, and seemed to occupy the cavity of the chest. Its natural situation also was completely changed; the right auricle and ventricle, or the heart which moves the black blood, was superior and turned towards the left side of the chest; while the left, or heart which moves the red blood, was towards the right side of the chest. The apex pointed to the right side. But this change was not confined to the heart, it extended to every organ of the thorax and abdomen. The three lobed lung was on the left, and the two lobed on the right side. The aorta descended on the right side of the spine, while the vena cava was on the left. The arteria innominata distributed the blood to the left, while the single carotid and subclavian were sent to the right. The œsophagus descended and pierced the diaphram on the right side; and in the abdomen the stomach and spleen had exchanged places with the liver. The intestinal canal partook of the same arrangement. The duodenum was on the left, as was also the secum; but the sigmoid flexure of the colon and the rectum were on the right side.

This change of place merely, would not, probably, have prevented the continuance of the individual, had the heart been as natural in its structure, as were the other organs. But in this a most eccentric arrangement presented. The aorta very small, arose directly from what is commonly the right ventricle, and rode over the root of the pulmonary artery, which had its origin in the opposite or aortic ventricle. The veins were connected with the heart in their natural manner; the blood of the cavas, therefore, was received into the same ventricle that gave origin to the aorta, and the pulmonary veins poured their blood into the ventricle, from which the pulmonary artery had its origin. There appeared to be established two circulations having no sort of connexion with each other; and it seemed that the venous blood must be distributed to the various parts of the body, without being subjected to the influence of the lungs. No communication existed between the aorta and pulmonary artery; the ductus arteriosus seemed never to have existed. But the foramin ovale was open and very much enlarged; through this the blood of each side must have mixed; it was the only way in which the body could have received one drop of arterial blood, and by which the short term of its existence was continued.

WILLIAM GAMAGE, Jr.

June, 20*th*, 1815.

Exposé of the State of Medicine in France, during the year 1814, *being the Anniversary Discourse of Baron Des Genettes, as President of the Faculty of Medicine, in Paris.*

[From the Medical and Physical Journal, April 1815.]

GENTLEMEN,

A YEAR has elapsed since one of our number, according to custom, opened the schools of medicine by taking a view of the progress of medical knowledge during the preceding year.

The circumstances of the world were then such as have rarely occurred in the annals of nations. All the military forces of Europe, humbled so many years by our triumphs, began to drive us back upon our own frontiers; innumerable armies subsequently passed them, and after many sanguinary battles they arrived at the gate of this capital, into which, however, they did not penetrate until peace was restored to France and to nearly the whole world.

Under such embarrassing circumstances, gentlemen, you never shut up your schools nor interrupted your functions. Uniting, almost all of you, the practice of the art to the teaching of its theories, you, in a manner, multiplied yourselves to relieve the victims of war, who are no longer enemies.

Those who have preceded me in this chair have introduced the most interesting subjects; they have exhausted all that can be said as to the extensiveness of our plan of instruction.

I shall, therefore, confine myself to the exposition of our labours, afterwards do homage to the memory of such of our colleagues as have been recently taken from us; and lastly, fix the public attention of their fellow-pupils on such of our students as shall receive the prizes this day awarded.

Baron Boyer, our colleague, has published in the course of the year, " A Treatise on Chirugical Diseases and Operations." The author of this important work thinks that the progress of modern surgery has been so rapid and remarkable, that it seems to have attained the highest degree of perfection of which it is susceptible. The greater part of surgical diseases are now perfectly well known, both with respect to their phenomena and the indications which they present; we may even frequently ascertain their proximate causes, determine their essential character, and, consequently, the best treatment. Instruments and apparatus are simplified from day to day, and the application of medicaments is better understood.

Therapeutics and hygiena which sometimes of itself supplies their place, and always aids them, belong to both branches of the medical art; the improvement of the therapeutic and hygienic methods, therefore, equally ensure the success of internal and external applications; a new proof among many others of the indispensable necessity of rendering common to medicine and surgery the same institutions, and consequently the same sources of instruction.

Improvements so great, admitted by the whole world, and the chief glory of which is cheerfully conceded to the French, have rendered necessary the new publication of an entire body of chirugical doctrine, to unite in a treatise of a size at once convenient and elementary, knowledge scattered in books which would form a library.

M. Boyer, after the example of some of the restorers and promoters of the art, divides surgical pathology into two great parts; the first is dedicated to the diseases which appear in every part of the body, and is subdivided into several chapters, in which he treats of inflammation in general; of abscess, gangrene, burns, wounds, tumours, ulcers, and the various diseases of the bones and articulations.

The second part, drawn up after an arrangement purely anatomical, embraces the diseases peculiar to each organ. Most of the diseases which require surgical operations, are referred to this division.

The author informs us, that it never was his intention to annex a complete treatise on operations, which would require immense labour. For these, he refers to particular works on operative surgery, more especially to that recently published by M. Roux, adding that the union of this work, with his own, will form a complete system of surgery.

On witnessing this association of a man of consummate experience with one who is still young, let those who are ignorant or pretend to be so, of the state of our present method of instruction, cease to indulge in absurd declamations! Let them no longer repeat in the face of direct evidence, that our school has never formed and never will form surgeons. We shall answer them by telling them, that Richerand, Dupuytren, and Desormeaux, were seated on the benches of this school, before they sat among its professors; and that the former students of this school, such as Ribes, Tartra, Marjorlin, Baffos, Beauchene, Murat, Beclar, Baren, Brechet, and several others, promise to be the worthy successors of those who are now at the head of French surgery.

On reading M. Boyer's work, as well for my own instruction, as to give an account of it at this meeting, I could not refrain from making some reflections on the chapter, entitled "On the Gangrene Produced by Freezing." An eye witness of the disasters which attended the retreat from Moscow, I had an opportunity of observing in an almost innumerable mass of men, the effects of the most rigorous cold.

I pass over in silence that perfidious sleep which fixes the body on a frozen bed and induces inevitable death.

The doctrines of M. Boyer are entirely conformable to my own observations. To describe our sufferings would be to copy his whole chapter. Let us merely quote a few lines:—"A sudden augmentation of very intense cold," says M. Boyer, "particularly when it is accompanied with wind, frequently occasions gangrenous affections and sudden death; we saw all this but too well verified. When on the shores of the Beresina, a very violent north wind covered our faces with flakes of snow; the thermometer then stood at 21° below the freezing point, and it fell, a few days afterwards, to 24°, and even to 27°."

In another place M. Boyer says, "it has been thought that the cold extinguished the vital action, merely by coagulating the animal fluids; but the phenomena which accompany freezing announce that the cold acts on the solids also, and particularly on the vessels and nerves. It acts on the former by diminishing, and even extinguishing their organic action; on the latter, by blunting their sensibility, and thus preventing the exercise of their functions." We may go further, and indicate a mode of action of cold little known, a very intense effect on the brain and nerves, even when congelation has not attacked any part of the body at a distance from the centre of circulation; we have seen men marching with every appearance of muscular energy, and the most decided and soldier-like pace, and heard them suddenly complain that a thick veil was covering their eyes; these organs, at first for an instant haggard, soon became immoveable; all the muscular apparatus of the neck, and more particularly the *sterno-cleido-mastoidei* muscles, became rigid, and gradually fixed the head on the right or left shoulder. The rigidity next extended to the trunk; the lower extremities then tottered, and the unhappy victim fell upon the ground, exhibiting, to complete the frightful picture, all the symptoms of catalepsy or epilepsy.

But to return to M. Boyer and his work; he has not entered into any details respecting the history of the art; he has not even indulged in a quotation, for this would not accord with his plan; he is anxious (I copy his own expression) to show what is proper to be done and not what has been done, upon this or that occasion.

A professor, who, like M. Boyer, has been long in the practice of lecturing, must occasionally find his own ideas in circulation; he has in fact recognized some, and has informed the public of them, that he might not pass for the plagiarist of those who have actually copied from him.

In a word, the work of which we have just given a slight sketch, is worthy of M. Boyer's high reputation.

Professor Richerand has also published, in the course of the year, the sixth edition of his Physiology. Although not essentially different from the former editions, and although the same arrangement is preserved, yet several points of doctrine have undergone modifications, corrections, and important additions;

some articles, which were formerly too concise, have received the desired extension; taking advantage, in short, of the labours which daily enrich physiology, M. Richerand has learned how to found them and to appropriate them to himself, so as to justify the title of new elements, which his celebrated work bears; the preceding editions of which, have been already translated into several languages.

M. Alphonsus Leroy, another of our colleagues, has published a little treatise, "On the Contagion which lately Raged among the Cows, Oxen, and even the Human Race, in some parts of France." He has therein discussed the causes of contagion, with the means of remedying and preventing them, and concludes by some reflections on the utility of extensive public lazarettos.

M. Leroy, in his ardor for the advancement of medical science, and more particularly that branch which relates to the health of the female sex, has had the boldness to advise, in a case of schirrous uterus, the revival of the operations first practised by Osiander, and since executed by M. Depuytren with all the dexterity for which he is distinguished. I merely indicate these subjects, leaving it to our colleagues to give the interesting details to the public, if they think proper.

Our colleague, M. Petit Radell, continues to be occupied in editing several articles for the *Encyclopedia Methodique.* We know that almost the whole of the surgical part of this grand dictionary has been executed of late by this laborious professor. We shall find, in succeeding numbers, several medical articles by M. Petit Radell; among others, those which relate to hereditary, moral, nervous, organic, rheumatic, soporific, and syphilitic diseases; we shall also therein find the article, "Ancient and Modern Physicians, as well religious as atheistical."

It could not be difficult for our colleague to prove the religious character of several of our professional brethren; but has he shewn, with equal clearness, that others among them have merited the reproach of atheism. Is not this rather one of those outrages inconsiderately committed upon great minds, by the intolerance of ignorant sectaries, or by the interested supporters of the most ridiculous superstition? We see, on the contrary, that even in the days of polytheism, the religion of the physicians, in conformity to reason, rose above vulgar credulity. How exemplary and beautiful was the worship of Galen, when, exploring the remains of men and the animals, the structure of which he was studying, he said to the gods: "I shall describe these wonders, and they shall serve as the hymns by which I shall celebrate your power."

The alarm, more or less well founded, relative to the introduction into the capital, of a contagious fever, which raged in several places where the armies were quartered and fought, or left sick behind them, gave rise to instructions which were drawn up by the faculty of medicine. I shall respect the modesty of those who drew up these papers, and omit, like themselves, to publish their names.

Many physicians and students of our faculty, have been sent to give assistance to the menaced and actually diseased departments; others have been of great service in the hospitals of Paris, while several have fallen victims to their humane exertions. Brilliant rewards have signalized the names of part of the survivors, and all have acquired just claims on the public gratitude.

Among the inaugural dissertations presented to the faculty, in course of the year, the following have been particularly remarked. I cite them in the chronological order in which they appeared.

1. Analytical Researches on Various Affections, in which the skin presents a blue colour, and in particular on those which have been designated by the name of cyanose, by M. Gintrac.

2. Dissertation on the Plague or Adeno-Nervous Fever, by M. Le Breton, of Rhodosto in Greece.

3. Observations intended to resolve the question, Is the Apoplexy which is Produced by an Afflux of Blood to the Brain susceptible of Cure? by M. Riobé.

We shall now speak of the labours of the members of the society of medicine, which we purposely confound with those of the Faculty of Medicine itself.

M. Alibert has given a new edition of his Elements of Therapeutics and Materia Medica; this work which was translated, as soon as it appeared, into several languages, has been long diffused and received among the most celebrated medical schools of Europe. Nothing can be more judicious than the course pursued by the author in this imperfect work, which is the result of much profound and well-directed study.

The Precis of M. Alibert, respecting mineral waters, has this year received many important additions.

His great work on the Diseases of Skin sustains its reputations; the ninth number, which treats of syphilitic affections, is full of striking matter; the rare perfection, which we see in the execution of the plates, gives a great interest to those objects, hideous as they are. We expect, with a lively impatience, the continuation of this magnificent work, in which scrofulous cutaneous diseases will be treated.

M. Roux, has published a "Memoir and Observations on the Immediate Union of the Wound after the Circular Amputation of the Limbs in their Continuity, and particularly after the Amputation of the Thigh," which were read to the class of Physics and Mathematics of the Institute, on the 21st of March, 1814. This paper, the object of which, is to discuss and revive from observations actually made, methods which have been rejected, opposed, or laid aside, has been sufficiently appreciated, and has received the encouragement which it deserves, in a report by Messrs. Percy and Deschamps, adopted by the Institute, and published at the end of their memoirs and observations for the year.

M. Roux has added a curious case of a divergent strabismus of the right eye, cured in an adult, who was affected with it from infancy.

We are of opinion, that concerning a deformity in an organ so sensible as the eye, not only are repeated experiments requisite, but there must also be a permanency in the relief or the cure, ere we can decide upon the merit of the method, and ascertain, if we may be allowed the expression, what is truly specific.

Since the publication of the work just announced, M. Roux has made a visit to London, and we are assured that he intends to publish some observations, in order to make us acquainted with the particular practice of the most eminent surgeons of that capital. Such communications must be very instructive when coming from enlightened and impartial men, who have had time to look calmly around them; it will be at once advantageous and pleasing to know the successors or rivals of Pott, John Hunter, Keate, and Blizard.

M. Nysten has published a "Dictionary of Medicine and of the Accessary Sciences," 1 vol. 8vo. This work cannot fail to be very useful to students.

And why should we omit an important work, composed by a young Spanish physician who graduated with our Faculty, and by the circumstances of the times, became, in some measure, one of our adopted sons?

M. Orfila, the gentleman in question, has this year published a "Treatise on Poisons from Mineral Vegetables and Animals." This general toxology is drawn up with a due regard to physiology, pathology, and medical jurisprudence. Such a work, put forth with the assistance of several experiments and accurate observations, ought to be the better received, as it was really a desideratum in medicine and jurisprudence. In fact, all preceding treatises are, without exception, incomplete, inaccurate, and far behind the present state of our knowledge.

M. Orfila, in the first place, carefully describes the physical and sensible characters of poisons in their simple and natural state, and afterwards indicates the chemical propertes of those substances, noting, with great exactness, the phenomena which they exhibit by means of the great number of re-agents.

M. Orfila afterwards shews the difference which poison, when mixed with food of various kinds, presents with the same re-agents. But, what most concerns our art and the public security, is the indication of the best methods of arresting the action or incipient effects of poison, and remedying the disorders which they have already produced on the animal economy.

We think it right to observe, that we have promoted of late years to the degree of doctor of medicine, several Spaniards of the greatest promise; they have sustained, in the midst of us, by the lustre of their talents, a valuable portion of the national honour, while the majority of their fellow-countrymen, full of the recollection of the valorous achievements of their ancestors, have broken the yoke which an execrable tyrant sought to impose on them.

The Bulletin of the Faculty and of the Society of Medicine, the editing of which is confided to professor Dumeril, has communicated to the world, more or less in detail, whatever has been most interesting to the profession; we may remark in particular, in a recent number, the extract of an observation on the ligature of the external iliac artery, communicated by M. Bouchet, chief surgeon to the Hotel-Dieu, of Lyons. This fact, with the manner in which it was brought forward, and the reflections which it occasioned in the society, remind us of the most flourishing days of the old royal academy of surgery.

I must now call your attention to the losses which we have experienced. We have already observed that the faculty of medicine, and the society formed in its bosom, are indissolubly knit together. It is thus, as a member of the society which forms part of our schools, that I shall express our common regret for the loss of one who honoured me with his particular regard—Charles Louis Dumas. When upon the death of Grimaud, cut off, from his professional career, at the age of thirty-seven, it was you, Dumas, who placed yourself in the ranks to succeed him; you saw, without regret, the crown adjudged to Fouquet, who, at the age of sixty-five, could no longer have any rivals. Another chair became vacant by the death of M. Sabatier, and the keeper of the seals of France, who honoured you with his esteem, wished you to fill it. Afraid lest it should appear that you were indebted to favour and not to your own merits, you scrupled not to re-descend into the arena.

The writings of Dumas may be classed under three different heads; the first belong to the institutions or elements, and are purely anatomical or anatomico-physiological; the second relate to practical medicine; and the third class is composed of various isolated pieces, such as memoirs, observations, discourses, and eloges.

We shall take a cursory view of these various productions, not according to the division which we have established, but in chronological order, which will best exhibit the progress and history of his mind.

Dumas made his debut in 1785, at the age of twenty only, by an inaugural dissertation upon "Life;" this first performance indicates the direction which his mind took and constantly preserved from that moment.

A short time afterwards, he was a candidate for a prize, proposed by the Royal Society of Medicine; it was required "to determine in what species and what periods of chronic diseases fever may be useful or dangerous, and with what precaution we ought to excite or moderate it in their treatment." The memoir published by Dumas on this occasion is a judicious selection of the opinions of the best authors. Before the age of experience we can only resolve similar questions in this way—the prize was divided between Dumas and M. Pujol, formerly a practitioner at Castress in Languedoc.

Dumas published, for the competition of the year 1790, to which we have alluded, a series of Theses, which comformably to the custom of the faculty of Montpelier, were composed, distributed and sustained, within the space of fifteen days.

In 1791 Dumas gave an edition of the "Complete Course of Fevers," of the late De Grimaud, in 4 volumes 8vo. conformably to an autograph copy specified in the testament of the estimable author. Dumas was anticipated a few months or weeks by a self-elected editor, who without delicacy published the same work in three volumes only; and in the same form the opinions of medical men are divided as to the merit of the two editions.

Dumas was associated the same year with M. Petit D'Arsen in translating an "Essay on the Nature and Treatment of Pulmonary Consumption," written in English by Thomas Reid. This work contains only one fact and its inductions; this fact is, that the vomiting arising from sea sickness has been known to give relief in incipient phthysis; the inductions are, that repeated emetics in small doses might be useful in the treatment in the same disease, and in the same stage. Without discussing the merits of the original work we may affirm that the translators, by adding a preface and notes, have succeeded in making a far better book.

Elected professor of Anatomy and Physiology in 1795, Dumas published in 1797, in 1 volume 4to. a treatise on Myology, after the manner of Winslow, Albinus and Chaussier; this work is entitled, "Methodical System of the Nomenclature and Classification of the Muscles of the Human Body, with descriptive tables for recalling their ancient names, their new names, situations, attachments, direction, composition, figure, connection and use." A dictionary containing the whole *synonymy* of the muscles terminates this treatise. It is an imitation and an extension of the work of one of our colleagues whom we have mentioned constantly, preceded by general principles and reflections on the formation of languages.

Dumas published in 1800, and in 4 volumes 8vo. his "Principles of Physiology, or introduction to the experimental, philosophical, and medical knowledge of the living man." He brings forward with address whatever was most important on this subject, grounding his opinions in preferences upon the writings of Stahl, Haller, and Grimaud. Another person who approached in some respects the first, who in point of erudition equalled the second, and who surpassed the third in every respect, (I mean Barthey,) was offended at the silence of Dumas, who in borrowing several of his ideas had forgotten to acknowledge their author. Barthey had a right to complain, and he did so with that air of authority which was peculiar to him, and which he but too strictly maintained in this discussion.

I pass over slightly the "Physiological Sketch on the Transformation of the Organs."—*Journal de Physique*, 1805 *and* 1806. To believe that the stomach can fulfil the functions of the brain, and *vice versa*, the brain those of the stomach, would be to return to the chimerical ideas of Paracelsus and Van Helmont. It was quite in a different spirit that Dumas published an observation, that does great honour to his talents, on the subject of an epilepsy rendered intermittent, and afterwards cured by the administration of bark.—*Journal de Medicine.*

Dumas pronounced in 1804 a Discourse on the Future Progress of the Science of Man. His hopes were founded on the continuation of the application of analysis, and the perfection of the faculty of observation.

A new edition of his Physiology as before, in 4 volumes 8vo. appeared in 1806.

The Eloges of Fouquet and Dorthey, published the first in 1807 and the second in 1808, are two papers which do equal honour to the head and heart of Dumas. He had, it is true, in praising the former, merely to repeat what medical Europe had proclaimed for thirty years; but he had to make known in the latter a man nearly of the same age with himself, a former competitor for the same chair, and a rival for glory in the same school; although Dorthey, in a more especial manner, cultivated other branches of the natural sciences, in which he has left a great name.

We now come to a period of his life when Dumas published a work more original, and more peculiarly his own, if I may be allowed the expression, than any of the foregoing. It is the best of his productions, and also the last.

The treatise of Dumas on "Chronic Diseases," is divided into four parts. In the first, he explains the essential phenomena of these diseases, and the differences which exist between them and acute diseases. The second part explains the theory of the formation of chronic diseases. In the third part, he examines the modifications produced in diseases, by age, sex, passions, and influence of climate. The last and fourth part exhibits the application of the distinction of the elementary affections to the treatment of diseases.

This work, although very extensive, remained imperfect until it was followed by a volume of developments and applications to a great number of observations. When Dumas wrote this treatise, which appeared at Paris in 1812, he enjoyed a high reputation, which could only be increased by it; he was surrounded by that consideration which his talents and his eminent social qualities merited; finally, he was loaded with academical, literary, and other honors of every kind, when he died at Montpelier, on the 3d of April, 1813, at the age of forty-seven.

We have still another heavy loss to deplore, and it is still more recent and more premature; we mean that of Julien Jean Cæsar Le Gallois, who died in February last.

Professor Dumeril has already published a very interesting account of Le Gallois, the son of an honest agriculturalist in easy circumstances; he received a liberal education: he gave early signs of talent, and on finishing his studies he felt a *penchant* for medicine which he sudied in the university of Caen, which reckoned among its medical professors, Chibourg, Le Canu and Rousell, and which had produced Vicq-d'Azyr, Thouret and Vauquelin. The revolution about this period assumed a most frightful shape. Those who still retained some sentiments of commiseration and some ideas of equity, and the young in particular, burning with indignation, ranged themselves under the standard of a party which has since been distinguished—that of *Federalism*. Le Gallois became, under these circumstances, one of the leaders of the students. It is useless, impolitic, and perhaps dangerous, to dwell upon those times of calamity; suffice it to say, that the party in question was crushed in Calvados as well as throughout France, and that Le Gallois, obliged to fly, hid himself first at Paris, where he was discovered; that he took refuge among the sciences, and was so fortunate as to be employed in the manufacture of salt-petre in a department at a distance from the capital. Upon the formatiom of the three schools of medicine he returned to Paris, where he was received as one of the pupils from the departments; among his fellow students he was distinguished, and began by fixing the attention of the learned world upon him by his Thesis for the doctorate on the following question: "Is the blood identically the same in all the vessels which it passes through ?" This production announced a man of science, who was determined to proceed in his studies by the thorny, but otherwise fertile, road of experience.

Le Gallois shortly afterwards took part in the discussions occasioned by the famous thesis of Boulet, who in an ingenious and erudite paradox, threw some doubt on the existence of Hippocrates.

Suddenly a grand idea struck Le Gallois and absorbed all the faculties of his mind, he sought for the solution of the boldest problem, for he sought nothing less than the discovery of the principle of life.

The history of the sciences exhibits to us the first chemists as almost all occupied for centuries with the transmutation of the metals and the universal panacea; they could neither create gold, nor prolong the life of men; and yet they enriched the arts with numerous useful processes, and medicine with several very powerful remedies.

Le Gallois did not succeed any more than they in determining in what life precisely consists, and perhaps it is not given to the feeble intelligence of man to discover the primordial laws of the great phenomena of our organization; but in seeking for the solution of a question still undecided, Le Gallois threw great light on several very important points in physiology; he is, in this respect, the most distinguished man which our school has produced since Bichat.

The labours of Le Gallois are contained in a work, entitled "Experiments on the Principle of Life, particularly on that of the Motion of the Heart, and on the Seat of this Principle;" this valuable collection of facts has produced a work equally important, viz. the report made on this subject to the first class of the institute by Baron Humboldt. so dear to the sciences on many accounts, and our colleagues Hallé and Percy; these gentlemen caused to be repeated before them, 1st, the series of experiments relative to the principle of the inspiratory movements: 2d, the experiments relative to the principles of the powers of the heart. But these subjects cannot be analyzed in the present discourse.

Le Gallois has also left a work on the teeth of the rabbit and the Guinea pig; on the duration of gestation in the latter animal, and on the relaxation of the symphysis pubis at the moment of parturition; the observations and experiments on these various subjects were made while Le Gallois was inquiring into the principle of life.

The result of all his inquiries and experiments relative to circulation, are also printed in the excellent article Heart, (*Cœur*) which Le Gallois supplied for the New Dictionary of the Medical Sciences, which reckons among its authors several professors of this faculty, and almost all the most distinguished physicians and surgeons of this capital.

Le Gallois who was qualified by his education and talents to practice either surgery or medicine, adhered to the latter branch of the healing art; he had been nearly a twelvemonth physician to the Bicetre; he had lived in Paris, and it was when proceeding on foot to his duty, as he frequently did, that he was attacked by a peripneumonia, to which he fell a victim in the beginning of February, last year, leaving an interesting family inconsolable for his loss.

We have to express our regret that we cannot detain you a few minutes longer, by detailing at full length the life of Villars, dean of the faculty of medicine of Strasburg, and associate of our society of medicine, who died on the 27th of June last.

The details of his life would exhibit to your view a man deprived almost from birth of all hope of acquiring the slightest knowledge of letters, you would see afterwards by what difficult bye-paths, and how with talents almost entirely flowing from nature, he attained an honorable place among the physicians, and particularly the most distinguished botanists of the day.

The intimacy of Villars in his youth with J. J. Rousseau, his zeal for the instruction of the numerous pupils whose minds he formed, his humane attentions to the sick as physician to a great military hospital, or a practitioner among all classes of society, will present some affecting traits to whoever shall draw up his elogé with all the extent it deserves.

Let us no longer withhold the recompenses which await those, who at the last exhibition of our practical school ought to be preserved as examples for the emulation of their fellow students.

This last exhibition has not been in truth so numerously attended as on other occasions, and the candidates have not given the same proofs of proficiency as in preceding years; he whose name was first called was a convalescent from a severe disease, contracted in the hospital when he underwent his examination; the other candidates who will receive prizes after him, whatever be the place which they occupy, are equally worthy of encouragement and indulgence, both on account of their exertions, and of the arduous circumstances in which they found themselves at the end of the last and the beginning of the present year.

Let us all now forget the violent and too long continued misery from which we have but just emerged, and listen to the consolations of the peace, which can alone soften down our past misfortune.

But what have we not to expect, gentlemen, from the advancement of medical knowledge under the reign of a prince, who finds so many beautiful examples in every page of the history of the kings, his ancestors and predecessors, the establishments which Louis IX, Francis I, Henry IV, Louis XIV, Louis XV, and Louis XVI, consecrated to letters and to humanity, are ever present in the memory, and engraved on the heart of Louis XVIII; he has come among us to restore and to preserve; surrounded by the councils of wisdom, he will preserve for us whatever is good, and will improve whatever can be made better.

Memoir on the use of the Epiglottis in deglutition, presented to the first Class of the Institute, on the 22d March, 1813.

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[Translated for the New-England Journal of Medicine, &c.]

The epiglottis, that fibrous cartilage placed between the base of the tongue and larynx, has always been considered by anatomists as a sort of valve designed to close the glottis at the moment of deglutition, that solid or fluid aliments may not pass into the trachea. No one that I know of has raised a doubt on the subject, and this opinion still obtains a general assent. I was myself of this opinion; but possessing that spirit of inquiry which a physiologist should never be destitute of, I resolved to satisfy myself by direct experiments of its truth, which to my knowledge has never been done. I made an incision in the neck of a dog, between the thyroid cartilage and the os hyoides. I drew the epiglottis through this opening, and cut it entirely off. As I had taken every proper precaution, the operation was very simple, and the animal appeared but very little incommoded by it. I reunited the external wound by means of the suture. About an hour after this operation, I offered the dog food, he took it as usual, with avidity, and to my astonishment swallowed it without the least difficulty, and with full as much ease as before the extirpation of the epiglottis; I thought that at least he would experience some difficulty in swallowing liquids, (these being known to be more difficult to swallow than solids,) but I found the animal drank without any apparent embarrassment in deglutition; a few drops merely passing by the small wound in the neck.

This first experiment naturally excited my curiosity. How happens it, said I, that neither the drink nor other food fall into the glottis during deglutition?

To clear up this point, I made a similar incision in the neck of another dog, I caught the epiglottis with a hook; I drew it outside in such a manner, that through the wound I distinguished the glottis and its motions, synchronous to those of respirations; I poured into the gullet of the dog a spoonful of water, which was followed immediately by a motion of deglutition, during which I saw the glottis closed completely at the moment of the ascent of the larynx. I poured another spoonful of water into the gullet of the dog, and again I saw the glottis exactly close itself at the instant the animal exerted the efforts of deglutition. I found that we excited the motion of deglutition, by passing the finger into the pharynx through the wound in the neck; this I did a great many times, I became convinced that the glottis closes itself with great exactness during deglutition, whether the animal actually swallows solid food or liquids, or from any other cause exerts the functions of deglutition. I then understood how a dog easily swallowed, notwithstanding he had been deprived of the epiglottis. This closing of the glottis can depend only on the contraction of the true muscles of the larynx. I wished to know what influence a division of the recurrent nerves which are distributed to these muscles would have on this action, and especially of the nerves which supply those muscles which directly move the arytenoid cartilage. Having, therefore, as in the preceding experiment in a dog, drawn the epiglottis out, so as to be able to observe the glottis and its motions, I divided the recurrent nerves, I next excited the process of deglutition, and was convinced that the glottis continued to close itself; it seemed however that the two lips of the glottis were applied to each other with less force and exactness. It was important to ascertain whether the dog continued to swallow without difficulty, notwithstanding the division of the recurrent nerves: I now extirpated completely the epiglottis, and united the wound by suture; I did not see the dog for some hours, at the end of which I convinced myself that he ate and drank with most perfect ease. It was evident, that in this experiment the approximation of the lips of the glottis is affected by the influence of the superior laryngeal nerves. I wished to know what would be the effect of the division of these nerves on the closing of the glottis during deglutition. I made this division in a very large dog, and I distinctly saw that the contraction of the glottis was not as in the former case complete. The edges of this opening approximated well at their anterior part, but a sensible separation existed between their posterior extemity. The two arytenoid cartilages were no longer in perfect apposition with each other, as is the case when the nerves of the larynx are untouched. To complete this experiment, I extirpated the epiglottis, and left the wound which it was necessary to make, to heal, and when it was entirely cicatrized, I examined the dog, and found he swallowed in general with sufficient ease; but it was apparent, that at times he experienced a little difficulty, which he discovered by two or three efforts at coughing. I had still another experiment to make; it was to divide in a similar animal the recurrent and the laryngeal nerves, to be satisfied of the influence which this double section would have over the motions of the glottis and the power of swallowing. I made this division in a dog, and I saw that every motion of the glottis had ceased. The animal would fain have exercised the motions of deglutition, but the glottis remained open. As in the preceding experiment I cut the epiglottis, and left the wounds in the neck to cicatrize.—I kept this dog at my house some time, where a number of persons who have seen him can testify that he swallowed with the utmost difficulty. In fact every draught of liquid excited a paroxysm of coughing, and it was much the same with solid aliments. Finally, I have now among the animals experimented on, a dog in which I have cut within a few days the nerves of the larynx, leaving the epiglottis perfectly entire, notwithstanding it is evident that the animal swallows with extreme difficulty; every motion of deglutition being followed by a violent paroxysm of coughing. I have tried these experiments in many species of animals, cats, rabbits, and Indian hogs, and the results have been precisely the same with the above. I have also examined the superior glottis of birds in the moment of deglutition, and have found that they resemble the glottis of the mammalia, which appears to me to explain better than has hitherto been done the absence of the epiglottis in these animals. I propose to examine with this view other animals which have no epiglottis, but as yet circumstances have not permitted this investigation.

The conclusion to be drawn from these experiments, with regard to the epiglottis, is very evident. It is clear that this part is not necessary to the integrity of deglutition, and that it ought to be deprived at least in part of the importance which has been hitherto assigned to it; that the principal reason why the food does not fall into the trachea, is that at that instant, viz. of swallowing, the glottis closes with the greatest exactness. But there is one other inference to be drawn from it, it is, that it is principally through the influence of the laryngial nerves that the contraction of the glottis takes place during deglutition. This circumstance has induced me to make some experiments, which I shall have the honour to make known to the class; they will serve, I think, to prove that if very frequently anatomical discoveries conduct to physiological truths, these may in certain cases lead to some unknown facts in anatomy.

If we believe the descriptions which are contained in the best anatomical works, the laryngeal and recurrent nerves are distributed in the same manner to the true muscles of the larynx; but after having dissected with great care a great number of larynxes of men and animals, it appears to me this is not at all the case.

The branches of the recurrent nerves appear to me to be distributed only to the posterior crico arytenoid, to the lateral crico-arytenoid, and thyro-arytenoid muscles. I have never found any ramification of this nerve which went either to the crico-thyroid or arytenoid muscles; whilst the laryngeal nerve is distributed to the arytenoid muscle, to which it gives a very considerable branch, and to the crico-thyroid muscle, to which it gives a filament less remarkable for its size than for its extent. This filament descends between the thyroid cartilage and the thyro-arytenoid muscle; thence between this cartilage and the lateral crico-arytenoid muscle; finally, it passes entirely into the crico thyroid muscle.

Sometimes instead of descending behind the thyroid cartilage, this filament stretches across a *canal pratiqué* in the thickness of the cartilage, and thus arrives at the muscle for which it is destined. This disposition is very common in the dog, but more rare in man. In many subjects, whatever care I took, it was impossible to find the filament of which I am speaking; but at the same time it appeared to me that the external branch of the laryngeal nerve, which as we know is chiefly destined for the crico-thyroid muscle, was much more considerable in size. The other filaments of the laryngeal nerves do not go to the muscles of the larynx, they are spread principally among the muscles of the epiglottis, and the mucous membrane which lines the entrance of the glottis; a great number pass very near the superior edge of the thyro-arytenoid muscle; but I have seen none which penetrated into the texture of this muscle. This disposition of the peculiar nerves of the larynx appears to me to coincide exactly with the results of the experiments I have above related. I think it allows us to conceive how the recurrent nerves being cut the constriction of the glottis takes place in a manner nearly complete; for as these nerves are not distributed as the principal agents of the contraction of the glottis, which is affected by the arytenoid and crico-thyroid muscles, it is clear that the division of the recurrent nerves cannot prevent the contraction of these last muscles. Finally, this distribution perfectly explains why the four nerves proper to the larynx must be cut in order that the glottis may remain open and immoveable. It may be thought that I could not have made these physiological and anatomical observations on the larynx without having made some relative to the voice, and the mechanism of its production; but I think these sufficiently important for a particular memoir, and I shall in a short time have the honor of offering one on it to the class. I conclude with some observations relative to the crico-thyroid muscle, which are a little foreign to the principle subject of this memoir, but which I have not thought it worth while to separate from it. To this muscle is attributed the power of tilting the thyroid cartilage forward, by which it is brought near the crioid cartilage; at least it is thus that the most distinguished anatomists speak of it. But if we reflect on the anatomical disposition, and of the parts it is designed to move, we shall not easily adopt this opinion. In fact, the fibres of the crico-thyroid pass obliquely up and down, behind and before from the thyroid to the cricoid cartilage; they are inserted upon the thyroid cartilage, very near its articulation with the cricoid, and this disposition must needs render the action of this muscle very difficult, since it can only act as a lever of a very short arm. The only circumstance which would diminish this difficulty would be great flexibility in the thyroid, and great stiffness in the cricoid cartilage. This is however by no means the case; the thyroid cartilage is retained in its place, either immediately or mediately, by a great number of muscles, and on this account its motion downward and its tilting forward [*bascule en avant*] is very difficult; the cricoid cartilage on the other hand is extremely mobile; it is held below only by the trachea, whose facility of stretching we are acquainted with; so that, reasoning anatomically, I should think that the crico-thyroid muscle, in place of being designed to depress the thyroid cartilage, it should have that of elevating the cricoid, which must have the same influence on the glottis. But I need hesitate no longer to say that experiment entirely confirms this assertion. When we raise the skin of the neck in such a manner that we perceive the crico-thyroideal vacancy, there is room to be convinced that in the formation of acute sounds, and above all at the moment of swallowing, the cricoid cartilage is raised so that its superior edge comes in contact with the inferior edge of the thyroid cartilage.

From the observations contained in this memoir, I think we may conclude,

1st, That the epiglottis is not indispensable to deglutition. 2d, That during deglutition at the instant of the ascent of the larynx, the edges of the glottis and arytenoid cartilages approach so that the entrance of the larynx is completely shut against the substance that is to pass into the œsophagus. 3d, That the recurrent nerve once arrived at the larynx only sends forth muscular branches to the posterior crico-arytenoid, the lateral crico-arytenoid, and thyro-arytenoid muscles; while the laryngeal nerve sends filaments only to arytenoid and crico-thyroid muscles. 4th, That the crico-thyroid muscle has for its office to give to the cartilage a motion of elevation, which, at the instant of deglutition is carried so far that the superior edge of this cartilage comes in contact with the inferior edge of the thyroid cartilage.

Case of the Perforation of a Constricted Urethra by a Cutting Instrument, with Remarks on the fatal Consequences of such an Operation.

BY J. WILLIAMS, Surgeon to the Portsmouth and Portsea Ladies' Benevolent Society.

[From the London Medical, Surgical, and Pharmaceutical Repository.]

In the Repository, vol. i. p. 458, is inserted a case of Stricture, the treatment of which is recommended as original and beneficial; but which, in the instance I have to relate, proves decidedly destructive. Mr. Bythell says, he succeeded in removing a stricture of some extent, by means of forcing a pointed metallic instrument through the obstruction, until the resistance was subdued; and that the patient recovered. The success of this case encouraged him to describe the operation, and to recommend its adoption in similar instances. I am well aware how impracticable it is to generalize from individual cases; yet the recital of particular practical results cannot but meet with commendation, as it conduces to further experiments, by which they may be better illustrated, and the propriety of their adoption or rejection estimated.

Success in a solitary case is scarcely sufficient to justify the adoption of the successful practice. However, I do not intend to draw from my objection an universal rule; on the contrary, the case I offer, as it is a fatal case, will surely, I trust, depress the importance of the practice recommended by Mr. Bythell, as much as his single record of benefit could exalt it.

It is not my wish to depreciate success; events must ever be the standard of practice; and I consider that it would be unfair to the gentleman whose success has been made public, to withhold from that public a parallel case, the issue of which was diametrically opposite to his own. For although I admit with Celsus, that a doubtful remedy has a claim to our attention in desperate cases; yet I must have extraordinary prospects of relief, indeed, to influence me to the use of any measure, in which there exists a possibility of its being the cause of the death of a patient, who without its application would have suffered an inconvenience only without direct danger.*

Theory is certainly the true foundation of practice; and experimental inquiry is of the utmost importance, when it is formed upon the legitimate basis of analogy. Every one knows that the incision of a part excites inflammation, and a deposition of coagulable lymph, which in a short space of time becomes organized matter. It therefore appears from theory alone, that the puncture of a diseased part would in general remain pervious but for a short time. By the exudation of lymphatic matter, its sides would very soon become consolidated; and, if in the urethra, would soon obliterate the canal. I must, therefore, agree with the Editors of the Repository, who remark on Mr. Bythell's case, that they consider it more as a lucky hit, than as a precedent for practice. Besides a fatal termination, as will appear in the recital of the following case, may render those who follow such precedents obnoxious to a verdict of "mala praxis."

CASE.

Quarter Master Franklin, of the Limerick Militia, was in possession of tolerably good health, with the exception of having a stricture in the urethra, which incapacitated him from making water, otherwise than in a very slender stream; when he placed himself under the care of a soi disant Dr. W——, an illiterate and unauthorized practitioner of Surgery. This charlatan, for a considerable premium, engaged to remove the obstruction. There was a small discharge of mucus, not an unusual symptom of stricture, from the urinary canal. From the information I procured, he had also, some years ago, been the subject of repeated gonorrhæas; from which source he justly inferred the formation of his stricture. He had not, by his own account, had any recent sexual communication, which might have rendered the nature of the discharge equivocal. He had been I believe some weeks under this person's management, when the operation I am about to describe was determined upon as a dernier resort.

The patient was placed on a bed, with his knees elevated towards the belly, holding a candle for the convenience of the operator. A tube formed from a catheter was then passed down the urethra until its progress was resisted. A metallic bougie with a sharp point was now thrust through the canula down to the obstruction; and then by repeated efforts pushed apparently through it.*

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* I advert only to the case I have undertaken to relate; for I cannot but admit, that strictures of the urethra sometimes occasion immediate apprehension, although they are, I believe, more frequently unaccompanied with danger.

Some hours after this operation the patient was seized with symptoms indicative of inflammation of the bladder and peritoneum. These increased in such a rapid manner, that the assistance of the medical gentlemen of the regiment, Surgeon Furlong and Assistant Surgeon Gason, was requested by the patient. Staff Surgeon Irwin and myself also visited him. I attempted to pass a catheter and bougie into the bladder, but could not move them beyond the stricture, although I believe a catheter was passed immediately after the operation. After the means likely to remove the inflammation, such as venesection, purgatives, and other antiphlogistic remedies were used, without the least benefit, the patient died on the seventh day from the performance of the operation.

APPEARANCES ON DISSECTION.

An examination of the body took place the day after the patient's death, at which the army medical practitioners before mentioned, and myself, were present. The urethra being slit open through its whole length, a stricture was observed, nearly three quarters of an inch in length, situated about three inches and a half from the exterior orifice of the canal. No perforation could be discovered in it, but at its anterior part we plainly perceived an angular puncture, which exactly corresponded with the point of a small trochar, such as is used in tapping the scrotum in hydrocele. A probe was introduced at this part, which by following the course taken by the sharp instrument, passed into an adjoining sinous cavity; and did not enter into the substance of the stricture, but glanced sideways into this sinus, which originated from that part of the urethra posterior to the obstruction.* On opening this cavity, its parietes were perceptibly lacerated and incised by the progress of the trochar. We could now comprehend how this instrument had passed through the sinus, until it came to its source at the hinder part of the stricture, when another thrust drove it into the membranous and cellular substance near the neck of the bladder. The opening into the bladder was likewise triangular; but much larger than that anterior to the stricture. The bladder was much thickened and collapsed. Its fundus displayed livid discolorations, the effect of gangrene; and in and over the surface of the internal coat were observed, lengthened flakes of pus, discoloured by blood. The prostate gland did not exhibit peculiar marks of disease. The omentum and the peritoneum were beautifully and minutely injected with red blood, the undoubted consequence of sympathetic inflammation. The parts (consisting of the bladder and penis) were dissected off from the body, and given to me, with the wish from the gentlemen present that I would lay these facts before the public.

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* A flexible metallic bougie, armed with the cutting extremity of a trochar; and a catheter, with the perforated end cut off, were the instruments employed. These the operator displayed to us, and described the mode in which they were applied, by way of illustrating the simplicity of his practice, and exonerating himself from the imputation of producing the symptoms of disease under which the patient now laboured. This explanation was made before the patient's death, to Messrs. Furlong, Gason, and myself, the patient himself bearing testimony of its truth.

The preparation has been examined by most of the medical gentlemen in this neighbourhood, and will always be at hand for the inspection of others who may desire to examine it. It will remain as a monument of the impolicy of the toleration of empiricism, and as a record of the dangerous consequence of misplaced confidence.

"Fide sed cui vide."

A coroner's inquest was subsequently instituted to inquire into the cause of Mr. Franklin's death, when, from the depositions of the medical gentlemen of the army, and my own, the jury found the following verdict:—

"Died in consequence of an unskilful operation."

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* This was an effort of nature to form a new passage where the urine was obstructed; and it was curious to observe the form of this sinus, its breadth in the middle, and its converging to a point at the outer end of the stricture From these circumstances it appears possible that the urine might have burst through the thin membrane, which separated this sinus from the urethra; and thus have wrought a cure. I think a small opening must have already existed, although it was not discriminated from the incision made at the beginning of the stricture by the trochar.

Experiments and Observations on the Influence of the Nerves of the Eighth Pair on the Secretions of the Stomach.

BY B. C. BRODIE, ESQ. F. R. S.

[From the London Medical, Surgical, and Pharmaceutical Repository.]

In a paper formerly communicated to this Society by Sir Everard Home, and since published in the Philosophical Transactions for the year 1809, some facts were stated, which render it probable that the various animal secretions are dependent on the influence of the nervous system; and this opinion seemed to derive support from some physiological experiments which were afterwards instituted by myself, and in which it was observed, that after the functions of the brain had been destroyed, although the action of the heart continued, and the circulation of the blood was maintained as under ordinary circumstances, the secreting organs invariably ceased to perform their office.

It has been attempted by former physiologists to determine how far the nerves are necessary to secretion; but there are considerable obstacles in the way of this inquiry, and no observations, that have been hitherto made, appear to throw a great deal of light on the subject. The only method which can be devised of ascertaining, by direct experiment, whether the nerves are really necessary to secretion, is that of dividing the nervous branches by which the glands are supplied. But this, with respect to the greater number of glands, is an experiment impossible to perform; and, with respect to others, cannot be executed without so much disturbance and injury to the other parts, as must render it extremely difficult to arrive at any positive results. Perhaps in future investigations some circumstances may arise which will enable us to determine more satisfactorily this important physiological question. In the mean time, as the labours of physiologists have hitherto contributed so little to this purpose, any facts that tend to its elucidation may deserve to be recorded; and I am therefore induced to lay before the society the following experiments which afford one example of a secretion being dependent on the influence of the nerves.

The stomach derives its nerves principally from those of the eighth pair, or the *par vagum;* and the same nerves, as they assist in the formation of the semilunar ganglions, contribute to the supply of the rest of the alimentary canal, particularly of the small intestines. In an inquiry which I had formerly instituted, respecting the functions of the stomach, I divided these nerves in the neck of a dog, for the purpose of ascertaining the influence which they possess on the secretion of the gastric juice; but I was disappointed in my expectation since the animals always died, in consequence of the disturbed state of the respiration, which the injury of the nerves occasioned before there was an opportunity of ascertaining the effect produced on the process of digestion.

I had formerly ascertained, that in a dog, poisoned by arsenic, there is a copious secretion of mucous and watery fluid from the mucous membrane of the stomach and intestines, which are in consequence found after death completely and preternaturally distended; and it occurred to me, that although I could not ascertain the effect of the division of the nerves of the eighth pair on the natural secretions of the stomach, it might be possible to ascertain the effect on a secretion thus artificially produced. With this view I instituted the following experiments:—

Exp. 1. The nerves of the eighth pair, with the accompanying sympathetic nerves, were divided in the neck of a dog, and immediately afterwards ten grains of arsenic were inserted into a wound of the thigh. The breathing became laborious, as is usual where these nerves are divided, and afterwards the same symptoms took place, as commonly arise from the poison of arsenic, with this difference, that there was no discharge of fluid either from the stomach or intestines. He died at the end of three hours and a half. On dissection, the stomach and intestines were found to contain only food and feces, there being none of the mucous and watery secretion usually met with in an animal which has been killed in the same manner. The mucous membrane of the stomach and intestines was highly inflamed.

Exp. 2. The experiment was repeated on another dog. He died at the end of nine hours; and on dissection the stomach and intestines were not found to contain any mucous or watery fluid. Their mucous membrane was inflamed.

Exp. 3. A dog, immediately after the division of the same nerves in the neck, was made to swallow two ounces of saturated solution of white oxide of arsenic in water. He died at the expiration of three hours. On dissection, the stomach and intestines were found slightly inflamed, and they contained no mucous or watery fluid.

In these experiments the animals died from the application of the arsenic, and the poison produced the usual symptoms, with the exception of the copious mucous secretion, which takes place in other instances, from the stomach and intestines. The obvious conclusion was, that this secretion was prevented in consequence of the nervous influence having been interrupted by the division of the nerves of the eighth pair; but as this injury always induces a disturbed and laborious respiration, it was desirable to ascertain how far this circumstance might have operated towards the production of this effect; and I therefore repeated the experiment; but with this difference, that the nerves were divided in such a way as not to interfere with the functions of the lungs.

Exp. 4. Having made an incision into the abdomen of a dog, immediately below the short ribs, I divided, by means of a bistoury, the stomachic ropes formed by the nerves of the eighth pair, where they are situated on the œsophagus, immediately above the cardiac orifice of the stomach. The wound was closed by sutures. The respiration was not in the least disturbed, but was performed as frequently, and with as much ease, as under ordinary circumstances. The animal was afterwards inoculated in the thigh with the white oxide of arsenic, and he died in a few hours after the application of the poison, with the ordinary symptoms, except that there were no fluid evacuations from the stomach or intestines.

On dissection, the mucous membrane of the stomach and intestines was found inflamed. There was no watery or mucous fluid in the stomach or small intestines. There was a small quantity of mucus in the colon.

The result of this being the same as that of the former experiments, we may conclude that the suppression of the secretions in all of them was to be attributed solely to the division of the nerves: and all the facts which have been stated sufficiently demonstrate, that the secretions of the stomach and intestines are very much under the control of the nervous system. We cannot indeed venture to deduce from them any positive conclusions respecting the necessity of the nervous influence to the secretions in general; but as forming one link in the chain of an interesting, but difficult, physiological investigation, the circumstances which have been mentioned, may, perhaps, be considered as possessing some value, and as worthy of being recorded.

It is proper to observe, that I have attempted to pursue the investigation, so as to ascertain the effect produced on the process of digestion, by the division of the stomachic ropes on the termination of the œsophagus; but various circumstances, which it would be unnecessary to enumerate, have prevented my proceeding in the inquiry, and seem almost to render it impossible to make any observations on this subject.—*Philosophical Transactions, Part I. for* 1814.

REVIEW.

Observations on the Laws Governing the Communication of Contagious Diseases, and the Means of Arresting their Progress. (Read before the Literary and Philosophical Society of New-York on the 9th of June, 1814.) By David Hosack, M. D. F. L S. Professor of the Theory and Practice of Physic and Clinical Medicine in the University in the state of New-York, one of the Vice-Presidents of the Literary and Philosophical Society, &c. New-York, Van Winkle and Wiley, 1815.

The subject embraced in this paper is of the most interesting kind, and in its discussion the author displays the learning and talents which he is well known to possess. His opinions are stated in the following passage, with which the paper commences:

"In July, 1808, I addressed to Dr. Chisholm some observations on contagion, or infection.* The object of that communication was, if possible, to narrow the ground of controversy upon that important and much contested subject. This I endeavoured to do, first, by showing that the distinction which had been proposed by some late writers, between contagion and infection, was unnecessary and fallacious; secondly, by dividing all diseases which are contagious, infectious, or communicable from one person to another, into *different classes*, according to the several laws which appear to govern their communication. These classes are three in number.

"The first embracing those diseases which are communicated exclusively by *contact*; as the *itch*, *syphilis*, *hydrophobia*, *&c.* which are never conveyed through the medium of the atmosphere.

"The second including those diseases which are communicable both by *contact*, or the near approach to the sick, and by the *atmosphere*, as *measles*, *small pox*, *scarlet fever*, *&c.* which are communicable in every season of the year, and in every climate; in a pure as well as in an impure air, though more readily by means of the latter than the former, and with which persons are rarely infected more than once in their lives.

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* See Edinburgh Med. and Surg. Journal, vol. V. p 247. American Medical and Philosophical Journal, vol. II. p. 14.

" Under the third class are enumerated those diseases which are only, in general, communicable or contagious through the medium of an *impure* atmosphere; the air being rendered thus impure by the decomposition of animal and vegetable substances, as in low, marshy countries; or by concentrated human effluvia, as in camps, jails, hospitals, or on ship-board; but the same diseases I alleged, in a pure air, in large and well ventilated apartments, when the dress of the patient is frequently changed, all excrementitious discharges constantly removed, and attention paid to cleanliness in general, are not usually contagious, or, under such circumstances, are very rarely communicated from one person to another.

" In this class I included the *plague*, *dysentery*, *typhus fever*, in its various forms, of jail, ship, hospital, or lake fever, and the *yellow fever*.

" I also remarked that these diseases, like many of the first class, may be repeatedly contracted; but that they are communicable, or otherwise, according to the condition of the air in which they occur, or into which they may be introduced: it was further observed, that the atmosphere thus impregnated by the peculiar virus emanating from the diseased body, becomes *assimilated* to the poison, or ferment introduced, and thereby is rendered capable of reproducing in others the same *specific* disease, whether it be the plague, dysentery, typhus or yellow fever. Such are the outlines of my first communication to Dr. Chisholm.

"In 1809 Dr. Chisholm did me the honour to reply* to my observations, expressing his entire approbation of the two first classes, but objecting to the third. After enumerating his several objections, he requests me to reconsider my third division, which appears to him to be the only objectionable one. This I have done, and now submit to this Society the result of a further examination of this subject, and a detail of the facts by which I have been led to my conclusion relating to the laws of communication, which I have more particularly assigned to the febrile diseases enumerated in the third class."

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* See American Medical and Philosophical Register, vol. II. p. 121.

The evidence adduced by Dr. Hosack is designed to show, first, that the diseases of his third class are contagious; and second, that they are rarely, if ever communicated, except in an impure atmosphere. To detail this evidence fairly, we should be obliged to copy almost the whole paper. We refer our readers to the work, which is before the public, and highly worthy their diligent perusal.

Whether those, who deny the contagious character of the diseases in question, will consider the matter settled, may perhaps be doubted. This paper, at least, presents a distinct view of the subject. We do not profess to accord with the learned Professor in all his opinions, yet we prefer to avoid committing ourselves on points, on which the greatest pathologists are divided. We leave to others, more conversant with them, to examine the doctrines he has advanced. In one respect our national pride is fully gratified. This work is not only written in a pure and classical style, but it is also free from all those personalities, reproaches and criminations, which have disgraced some of the writings on the same subject on the other side of the Atlantic.

We avail ourselves of this opportunity to congratulate our countrymen on the formation of the Society, under whose auspices this publication has appeared. This Society was established in New-York about two years since, and is composed of some of the most eminent and learned men of that city. Such a Society was wanting there to combine the efforts of these men, and it is, we trust, destined to promote the best interests of science and humanity, and thereby to elevate the American name.

Some Cursory Observations on the Ordinary Construction of Wheel-Carriages, with an Attempt to Point out their Defects, and to Show how they may be Improved; whereby a saving may be made in the power applied, the motion be rendered more uniform and easy, and the danger of upsetting most effectually prevented; with appropriate engravings. By Horatio Gates Spafford, A. M. &c. &c. Albany, E. & E. Hosford, 1815.

HAVING received the above pamphlet in our editorial capacity, we felt an obligation imposed on us to notice its contents. A doubt only arose with us, how far a treatise upon wheel-carriages could come within the scope of our Journal, or be considered auxiliary to medical science. Recollecting, however, that to valetudinarians and convalescents, a comfortable vehicle is an object of no small consequence, in which air and exercise can be obtained without jolts and bruises, we have ventured to insert a short account of the work before us.

The alleged improvement in the construction of wheel-carriages, consists in the introduction of larger wheels than those now commonly in use, with a scheme for remedying the disadvantages which must necessarily arise from their increased dimensions. That the reader may comprehend the nature of this improvement, we give him the words of the author, which, in the original, are rendered still more intelligible by an accompanying plate.

"Reason, science, and philosophy, having long since taught every body that high wheels for carriages move much easier than low ones, the fact, as it will not be questioned, needs no examination here. That it is so, is beyond a doubt. Various attempts have been made to avail ourselves of the advantage of considerably higher wheels; and we accordingly see that, wherever it can be done without a counterbalancing inconvenience, wheels of a greater height are substituted for the lower ones of former times. If still higher ones than we now see have not been used, it is solely because no plan has been devised to remedy the inconvenience of too high a line of draught for the horse, and the increased height of the load or body of the carriage. For it is also as generally believed that the line of draught should be, what may be termed a lifting line; that is, that the horse's breast should be rather higher than the level of the line of draught. This is undoubtedly correct, and I believe there ought, for his ease, and to enable him to exert his full power, to be a greater difference than there commonly is in practice; this however, would require the wheels to be still smaller. To all these difficulties and objections to the use of high wheels, we may add another, and that of commanding influence in the construction of pleasure, and other carriages designed for rapid motion. That as we extend the height of the wheels, we increase the exposure to upsetting, and the danger attending it; for as the higher the wheel, the longer is the lever on which its motion acts; and as the axle, on which the load rests is necessarily in the centre of the wheel, it is evident that the higher the wheel the greater is the liability to upset.

"Such, I believe, are the reasons why the wheels of carriages ought, for the ease of the horse and for the ease of the rider, to be made much higher than those in common use; and such the objections to any increase of height. Perhaps it may be admitted with propriety, that the height of wheels now employed, is about that medium which best combines every practicable advantage, with the fewest disadvantages. But, when I say this, I mean to be understood as saying it of the present mode of constructing wheel-carriages with straight axles, which necessarily raise the load above the centre of the wheels. For while I anticipate the fullest conviction of truth and reason in all the above premises, I expect, as a natural consequence, that, pointing out a complete extension of every desired advantage, with a remedy for every disadvantage, the improvement will be eagerly adopted, or, at least, that reason, science, and philosophy will accord to it the meed of their approbation.

"Suppose it practicable to increase the height of the wheels of a chaise, for instance, to any desirable extent, and that not only without any inconvenience to the draught, or the height of the load, but with an actual advantage from the manner of appending it. This would really be pronounced the grand desideratum, while, as it never has been done, it would, perhaps, as readily be pronounced impossible. And suppose it should be added that all this could be effected, and in such a way, as most effectually to guard against the danger of upsetting? But, although I am confident of having effected all this and more, let me be cautious how I promise too much for belief, for it is a great deal, and proceed to show what I have done and how I have effected it.

"It will have been perceived that I propose a considerable addition to the height of carriage-wheels; and to remedy the inconvenience arising from this, I use a crooked, or what the coachmakers propose to call a cranked axle, such as is shown in figure 1, resting the load on the crank, below the direct or right line of the axes of the wheels. The figure will be perfectly understood, and words may be spared. The height of the wheels is seven feet, and the crank of the axle fifteen inches. The shafts attached to the axle at *a a*, are connected with it by the joints *e e*, which allow the axle to move freely back and forth when the carriage is in motion, and to adjust itself to the various positions required by the laws of gravity. But this is more particularly explained in figure 2; and I will only observe here, that while the carriage is in motion, on roads of the common unevenness of surface, this crank will be constantly changing its actual position, as respects the axes of the wheels; because its relative position to these axes, is all the while adapting itself to a kind of compound line of gravity, by which it will hang nearly in the direction of that line. This line of gravity is always intermediate between the horizontal line of progression and the natural perpendicular of gravity, and results from the well known laws of gravity applied to moving bodies. In the case of carriage-wheels, it may be called a compound line of gravity, resulting from the nature of their peculiar motion. It never falls perpendicularly under the axes, only when the wheels are at rest; and then the crank in the above axle, which sustains the load, hangs perpendicularly under them. If the load be light, and the motion slow, over an even surface, this variable line of gravity will fall but little forward of the perpendicular; but always some, as shall be explained. If, on the contrary, the load be heavy, the motion rapid, and the road uneven or rough, the line of gravity will fall very considerably forward of the perpendicular, and the crank will hang forward in that direction. On ascending a hill, where the resistance is great, even though the motion be slow, the crank hangs forward in the same proportion; that is, always according to the resistance to the motion of the carriage, and the power applied to overcome that resistance."

Our object in this review is to shew, that none of the proposed advantages can possibly be obtained in a carriage of this construction; but that, on the contrary, a great increase of labour for the horse and inconvenience for the passenger, must constitute its inevitable effects.

An important point, of which the author seems not to be aware, is, that the force employed by a horse harnessed in the common manner in drawing forward a wheel-carriage, must always be exerted in a direct line from the horse's breast to the centres of the wheels, or to some point between them, through which a straight axis would pass. This will be the case in whatever manner he is connected with this centre, whether by a simple or complicated apparatus, by a direct or a crooked line. The wheels roll forward in consequence of the forward pressure of the axletree against the inside of the naves or hubs. It is at this point, therefore, that the chief resistance is to be overcome and the horses strength, of course, exerted. It is of no consequence, whether the shafts and traces pass directly to a straight axle, or are connected by a lower course with the depending part of a cranked axle. The resistance to be overcome will always be the same, unless the author can prove, that a weight may be drawn forward more easily from the same point by a crooked stick than by a straight one, or that a sturgeon will weigh less, suspended at the point of a fish-hook, than he would if hung at its centre.

The joint connecting the shafts with the crank of the axle can be of no possible service, since it only renders the angle between them variable, and the connexion between the horse and wheels more or less direct. Some material disadvantages arise from it, as we shall presently see.

In regard to what the author calls a "compound line of gravity," and by which he appears to mean a diagonal between the direction of gravity and the force applied by the horse; this has very little to do with the progress of the carriage, since the carriage itself, considered as a whole, never moves in this direction. Now as it is the business of the horse to draw forward the whole carriage, he must put forth sufficient strength not only to swing forward the body of the carriage and axletree, but likewise to induce the wheels to follow; it being necessary in a progressive body, that every part should pursue the course of the rest—a fact, of which Hudibras appears to have been sensible, when he wore but one spur, arguing that if one side of the horse could be made to trot, the other would not hang behind.

We have now to take notice of a number of obvious and very essential inconveniencies, which must arise from the construction of a chaise of the form proposed by the pamphlet before us.

1. The increased size of the wheels renders the line of draught from the centre of the wheels to the horse's breast a descending line, forming an angle with the surface of the ground, thus bringing the surface of the ground to act against the progress of the wheels, and to present the same resistance as if the carriage were continually moving up hill.

2. The joint between the shafts and axle causes the axle to hang forward, on a common level or ascending road; whence it follows that the weight of the carriage will be carried forward to a point between the centre of the wheels and the horse, or in

other words a portion of the weight will be transferred to the horse's back. This portion will be increased upon the occurrence of every obstacle which causes a temporary stoppage or retardation of the progress of the wheels.

3. From the nature of the connexion of the crooked axle with the wheels, whenever it swings forward, it must also move upward, occasioning an extra jolt to the body of the carriage. If the wheels when in motion meet with a considerable obstacle, suppose a log of wood, the course of effects will be as follows. The wheels will stop or lose a part of their momentum, the joint will be stretched out by the exertions of the horse, and the body of the chaise carried forward and upward, elevating the passengers into the air. When the horse has put forth sufficient strength to overcome the resistance, the wheels pass over the obstacle and shoot forward to overtake the body, their arrival being announced by a sudden sinking of the carriage to the lowest point permitted by the axle, and at the same time by a vibration backward and forward occasioned by the moveable connexion of the axle with the wheels.

As we do not learn from the pamphlet before us, that any carriage has been constructed on the plan of this improvement, or that its advantages have been experienced in any other way than in fancy; our readers must be content with the short imaginary ride we have given them. If this has not fatigued them, and should they ever meet with an opportunity of testing practically the merits of the *improved* wheel carriage, we will venture to predict that they will pronounce the plan to be admirably promotive of exercise both to the horse and rider, but very little conducive to the ease or convenience of either.

Remarks on the Reply to some Remarks on the Review of Le Gallois on the Principle of Life.

It never was the intention of your correspondent to become the advocate for any peculiar expressions of M. Le Gallois, nor for any of his opinions, further than they appeared to be fair deductions from correct experiments. It is of little consequence to the main question in dispute, what may have been the particular meaning affixed by M. Le Gallois to certain phrases. But we can hardly do him greater injustice, than to suppose that he intended to prove, that animal sensations and voluntary motions existed in the trunk after the severance of the head.

In an attempt to explain my view of the meaning of M. Le Gallois, a proper distinction between the functions, sensation and motion, and the properties on which they depend, appears to me to have been maintained; but a different impression would be given, not intentionally, I am persuaded, by the manner in which the words are quoted in the reply.

I used the phrase, *organic sensibility*, after Bichat, to represent one of the properties observed in the muscular fibre. I adopt, in its place, the word *irritability*, without hesitation. These properties, irritability and mobility, I recognize as properties of the muscular fibre. But these are not inherent in the muscles, but are supplied to them by the nervous system. This position appeared to me to be proved by the experiments of M. Le Gallois. The phenomena produced by these experiments seemed to admit no other explanation. The most prominent of these, is the immediate loss of those properties by the muscles, as soon as that part of the nervous system, from which they receive their nerves, is destroyed. This effect is most conspicuous in the heart, the action of which immediately ceases on the destruction of the medulla spinalis, with the whole of which it is connected by the sympathetic nerve. The reviewers of M. Le Gallois did not consider the evidence of this derivation of the powers of the muscles, from the nervous system as conclusive; and proposed to account for these phenomena by the principle of sympathetic relation, which may exist between the medulla spinalis and the muscles, particularly the heart. This opinion was controverted in the remarks, and the views of M. Le Gallois attempted to be maintained. The reviewers demand, if we advert further to the subject, that we produce more facts. The burthen of proof, say they, lies on the supporters of the doctrine of Le Gallois. To this we do not object; we have not shrunk from the task. But, besides this, they advance plausible suppositions to invalidate our facts, and by ingeniously placing us in the negative, require us to prove them inadequate or admit them as valid. It is fair, say they, to presume that the properties are possessed by those organs which exercise them, unless it can be proved that they are derived from other organs. Those who take the negative in such case, are bound to prove the supposition incorrect. Now, on the contrary, it would seem the duty of any one assuming as fact, what is not well proved, to subject his assumption to the most severe criticism, and to surround it with all the proof of which it is susceptible. In this way only will a writer avoid misleading those readers, who place confidence in his assertions.

If the gentlemen had tested their opinions by a single experiment, they would not have found such a difference between the effects of dividing the nerves going to a part, and of destroying that part of the medulla, which supplies those nerves. The fact is, there is no difference as to the effects of these two operations. If all the nerves belonging to a part, a limb for instance, be cut at their origins, death will take place in that part as absolutely and almost as quickly, as if the medulla furnishing them had been destroyed. This phenomenon will not, it is presumed, be ascribed to the power of sympathy. It seems to afford a direct proof, that the muscles are dependent on the nervous system for their vitality.

Another phenomenon, of a character equally decisive, may be stated. If the heart be taken from an animal immediately upon stopping its respiration, it will continue a weak pulsatory motion from eight to ten minutes. But if the heart be permitted to remain attached to the trunk, the chest being laid open, it will be seen to beat with force at first, and to continue its motion at least from twenty to thirty minutes, even if it be entirely emptied of blood at the commencement, by cutting some large vessel. The circumstances under which the heart is placed, in both cases, are similar; but the difference in the display of vital properties is striking; and there appears no other cause for this difference, than the presence of the nervous influence in the latter case, Further, if the spinal canal be laid open, and this is done without much difficulty in new born animals, and the nerves are cut off at their origin, without injuring the medulla, the heart will die almost as speedily as if separated from the body.

"We are authorized by analogy, by a knowledge of the similarity of principles employed by nature in effecting similar purposes, to believe that the property we call mobility in animals is the same, as that possessed by the vessels and some other parts in plants. Now if we suppose this property to be communicated to the parts, which exercise it, from the nervous system in animals, we must suppose that it has a similar origin in vegetables."

This quotation expresses, most aptly, the sentiments of your correspondent. Now he did, most certainly, believe it to be proved, that this property was communicated by the nervous system in animals, and nothing has yet transpired to weaken his faith; and therefore he did not draw any conclusion with respect to the existence of this system in vegetables, which was not strictly justified by analogy, according to the spirit of the above quotation. "But a nervous system has never yet been discovered in vegetables; and there is not any function exercised by them, which manifestly requires such a system." Now it has not yet been proved, that no such function is exercised by them, which requires such a system. On the contrary, should conviction one day be irresistible, that it is a function of the nervous system to produce the property *mobility*, the admission that vegetables did exercise such a function, as required such a system, would be absolutely necessary. For it is admitted, that this property is the result of a function exercised both by vegetables and animals; but this is ascribed to the nutritive vessels, with how much justice I shall endeavour to examine in another place.

The validity of the doctrine, that the heart derives its power from the nervous system, is next examined in the reply. The fact that the heart immediately dies in consequence of the destruction of the medulla spinalis, is not considered as proof that its power is derived from that source. The phenomenon, it is thought, may be explained upon the principle of a sympathetic relation. This explanation cannot be claimed, except the medulla spinalis be destroyed suddenly. We endeavoured to show in our remarks (page 6 of the Journal) and we think we did show, by their own mode of reasoning, that if the medulla be destroyed slowly and gently, the heart ought not to be affected, supposing it to have an independent power. The sympathetic relation, which may exist between the heart and medulla spinalis ought to be governed by the same laws, as the same relation between the heart and any other organ. Now let it be admitted, that the sudden destruction of the brain and stomach will sympathetically cause the death of the heart; it is well known, that by the slow destruction of these organs this effect will not be produced; the heart will, in fact, suffer very little. The same result would be likely to follow the slow destruction of the medulla spinalis.

But will even the sudden destruction of the brain or a blow at the pit of the stomach cause the death of the heart by any direct influence on that organ? To assist our judgment on the first point, I made the experiment recommended. The os occipitis of an animal was cut up, and the brain suddenly destroyed with a large instrument. Artificial respiration was then commenced, and the result was the same, as when the cervical medulla is is only divided, and the animal was used for other experiments. It will be well for the gentlemen to repeat this experiment. Whether the animal be saved from a watery grave or not, it will not alter the result It will cost but little labour, and it may do much towards convincing them of the error of their theory. A blow on the pit of the stomach, it is said, on the authority of Mr. Hunter and Mr. Adams, will not merely suspend the functions, but produce immediate and absolute death. It is better, generally, to appeal to one's own senses than to the authority of any man. I am pretty well convinced from the result of several trials, that an animal, a quadruped at least, cannot be immediately killed by a blow at the pit of the stomach, unless it be so severe as to lacerate some important organ in such a manner, that the animal would die from hæmorrhage. Mr. Adams, it seems, was not very fortunate in his attempts to kill animals in this way; he succeeded but once in many trials. In that instance, we are not told the manner in which it was accomplished, nor by what instrument. We do not know the extent of injury, which any organ suffered. I beg leave to state the following experiment:

A young animal received a smart blow at the pit of the stomach from the knuckles, directed against the diaphragm and stomach; the animal cried once, and respiration was suspended for about two minutes, when it began to recover. The blow was smartly repeated several times with the head of a cane, in the same direction. The animal did not breathe, and was evidently dying; artificial respiration was commenced, but without effect; there was no pulsation in the carotids. In ten minutes after the infliction of the last blows, all motion ceased in the limbs. The chest was laid open. The heart was still in motion, and continued to move feebly for eighteen minutes. Much extravasated blood presented, some of it coagulated. The inferior lobe of the right lung was extensively lacerated. The abdomen also had in it much extravasated blood, and the liver was lacerated. The stomach was empty, and no certain marks of violence appeared on it, nor on the diaphragm.

Notwithstanding the violence and repetition of the blows in this case, death did not take place in the heart, nothing like immediate and absolute death, but a suspension merely of its function; and this might undoubtedly have been restored, had there been no rupture of the lungs nor of the blood vessels. Death was evidently in consequence of the hæmorrhage, and the heart died, as it usually does from that cause.

If then, neither a violent and rude injury of the brain, nor a blow at the pit of the stomach will cause an annihilation of the power of the heart in consequence of any sympathetic relation, we cannot analogically, from these causes, apply the principle to explain the phenomenon, when it takes place from the sudden destruction of the medulla spinalis; and the conclusion, that the cause of this phenomenon is the loss of the source of this power of the heart, returns again with increased strength.

It is conceived to be a difficulty in this doctrine, that the actions of the heart are not arrested by the destruction of any particular portion of the medulla spinalis. An explanation of this difficulty is to be found in the fact, which was mentioned in the remarks, that the heart being connected by the sympathetic nerve with every part of the medulla spinalis is not dependent on any particular portion; each and every part contributes its aid in furnishing the heart its power, so that, so long as any portion remains uninjured, the heart is supplied, according to the extent of the uninjured part. The force of the actions of the heart, however, is diminished in proportion to the loss of its source of power.

The mode also, in which the medulla spinalis is injured, it is thought, influences the effect on the heart. But it is easy to be convinced, that this is rather apparent than real. It ought to be kept in mind, that there is not the least difference in the result, when the whole medulla is destroyed; whether it be done suddenly or slowly, and with the utmost gentleness, the death of the heart follows as certainly in the one case as in the other. When a portion only of the medulla is destroyed, there is reason to believe, that the effect on the heart is in proportion to the extent of the injury. If an iron wire be slowly introduced into the spinal canal, the injury to the medulla will probably be of the extent to which the instrument passes; but if it be pushed suddenly and with violence, the injury may extend very far beyond the point of contact of the instrument, and even to the disorganizing the whole remaining medulla; and the effect on the heart would be much greater, than what should arise from the apparent injury. This was probably what happened to the rabbit killed by the poulterer, referred to, as related in the "Preliminary Remarks." To give strength to this view of the subject, the following experiment was undertaken.

The spinal marrow of an animal was divided in the usual way, and artificial respiration performed About the extent of the cervical portion was then separated from the medulla, and a guard fixed at this place. An iron wire was then introduced between the occiput and the vertebræ, and pushed suddenly down to the guard. The cervical portion was thus destroyed, but the shock was prevented from extending to the remainder of the medulla. The heart continued to carry on the circulation, though with diminished force. Another portion was destroyed in the same way, with the same precautions, and with similar effect. After waiting five minutes, the remaining portion was destroyed, and the circulation immediately ceased.

It is granted in the reply, that the vital properties are the result of the exercise of the functions of living lungs; but the function of producing the properties, mobility and irritability, is assigned to the nutritive vessels. "The parts of living beings receive their structure from the extreme vessels, and there is no difficulty in supposing, that they receive their vital properties from the same vessels at the same time."

First, No advantage is to be gained by taking this function from the nervous system, some parts of which have little or no consideration in our systems of physiology, and imposing it on the nutritive vessels, which are already well burthened with duty.

Second. The evidence that these vessels perform this function is defective. The facts that are at present known, go to prove, that it belongs to the nervous system. For instance, a part, in which the circulation of the blood has ceased, but which retains its connexion with its source of nervous influence, will manifest these properties for a much longer time, than a part which is deprived of this connexion. The head of a new-born animal, separated from the trunk, will retain these properties for at least ten or fifteen minutes; but from an amputated limb, they disappear in less than five minutes. The circumstances of these two cases, with respect to the action of their nutritive vessels, are the same; and if the properties were derived from these vessels, they ought to be equally evident during the same time in both. To these facts, the result of the following experiment adds weight.

I tied the external iliac artery of the left limb of an animal, kept alive by artificial respiration, and cut it off. I also cut through all the blood vessels, that supplied the back part of the limb, but left the nerves entire. The limb continued to move whenever it was irritated, though no blood was carried to it. In the right limb every nerve was divided, but the blood vessels left entire. After three minutes, no motion could be excited in this limb. The circulation was maintained in the trunk one hour, and the limb in which the nerves were left, retained its irritability and mobility to the last. These properties could not have been derived from the nutritive vessels in this case; for they being deprived of the blood, had no materials from which to elaborate them.

"If M. Le Gallois' doctrine be admitted, it does not appear from what other source than its nutritive vessels, the medulla could derive its properties."

Every living being, as soon as it begins to exist, we must suppose, has in miniature every important organ ready to perform its function, as soon as it is brought into action by the application of stimuli. The seed and egg, for instance, contain the rudiments of every part of the future plant and animal. Among these, in the animal, the nervous system is first in importance, and, as a necessary part of this system, the spinal marrow will be present. Its existence is derived like other parts from the parent, and from the same source it has its properties. If its function be to furnish to the several parts of the being, in their growth and increase, the properties, irritability and mobility, it certainly will be capable by its peculiar vessels, adapted to this purpose, of enduing the new particles, added to its own substance by its nutritive vessels, with the same properties.

"It is very certain, that the brain is not the source of the property called sensibility, any more than the trunks and the nerves are such a source." It will not be denied, I presume, that sensibility is produced, as are other properties of the animal; whether by the brain or nerves, it does not seem to be certainly ascertained. But there seems no great error in assigning the function to the brain, since we have no evidence of the presence of the property in any part, when the communication between the brain and such part is interrupted. Some experiments have been instituted to ascertain, whether the nerves were mere conductors of properties from the brain and spinal marrow, or whether they also performed a similar function with these. The evidence, so far as it has come to my knowledge, is against the latter position. Take this consideration for instance, taking it for granted, that the property *mobility* is derived from the nervous system; if the nerves perform an office similar to that of the organ whence they originate, why do they not continue to perform such office, after their separation from the source of their origin! In the case before related, the nerves received no other injury, than being thus separated; they were also supplied with blood, but no motion could be excited in the limb. The reason is obvious, if the nerves be only conductors.

The last subject adverted to in the reply, is our difference respecting the action of the muscles of respiration. The reviewers considered it, for the most part, involuntary and the effect of sympathy. In the remarks on the contrary, it was contended that this action was principally, if not altogether, voluntary. The reasons for this belief are not controverted, and no new evidence offered in the reply in support of the other opinion, except it is affirmed, that voluntary respiration is an extremely laborious and imperfect act. This, however, cannot be readily granted. Are the acts of speaking, singing, &c. so very laborious and distressing? Are not these performed by the power of volition? Let any man sit down and carry on respiration voluntarily, and say if it be so difficult an act. Why then does the animal, in which the par vagum is divided, perform this act so slowly and with so much labour, as described by M. Le

Gallois? I answer, it is not because involuntary respiration ceases, nor because voluntary respiration is so distressing an act; but because, after the division of the par vagum, the lungs cease to perform their function; their cells begin to be filled with a bloody and frothy matter, and the necessary change in the blood is not effected. The animal, conscious of some defect, is influenced to make great efforts to get relief, by increasing the volume of air in inspiration; for which purpose, he enlarges the capacity of the chest, makes slow and long inspirations, and endeavours to retain the air as long as possible. In cases of difficult respiration in man, there is no reason to suppose, that any sympathy which had existed between the lungs and the respiratory muscles had ceased; the chain of this relation is not broken; involuntary respiration then surely does not cease; this cannot be the cause of the difficulty. Some defect in the lungs themselves appears to be the most common cause, and the violent efforts which are made, seem to be for the purpose of obviating the consequences of the imperfect action of these organs by inspiring a greater quantity of air.

A too frequent reference to the same subject, it is apprehended in the reply, may be wearisome to some of the readers of the Journal, and a wish to close the farther consideration of this, is intimated. The subject is new, and as it respects M Le Gallois' doctrine concerning the function of the medulla spinalis, of great interest. It is not, however, my intention to excite further discussion, and the publication of this paper will not be desired, if the facts related should not be thought of sufficient interest to be worthy of record. W.

Boston, June 15*th*, 1815.

Note by the Editors.

We give our correspondent credit for his perseverance and ingenuity. New experiments may authorize new deductions. Those drawn from the facts stated by Le Gallois, rest on their own basis. For the rest, it may be well to wait for the confirmation of the facts relative to the subject, and such new ones as time may develope, before deciding on the principles under discussion.

INTELLIGENCE.

We understand that the promised work of Dr. Gallup is just ready for publication, the sheets being already printed. This work is entitled "*Sketches of Epidemic Diseases in the State of Vermont, from its first Settlement to the year* 1815; *with a consideration of their Causes, Phenomena, and Treatment. To which are added Remarks on Pulmonary Consumption.*" The subject of this work is full of interest. Diseases in the different parts of New-England do not perhaps vary materially in their causes and characteristics. Any information respecting them must be regarded with great favour by the members of the medical profession, and by the public at large. There is reason to believe, that in this case the information is derived from a source worthy general respect and confidence.

Among the candidates who have lately appeared for the palm, which is to reward the discoverer of a specific for gout, C. Wilson, M. D. of Yoxford, Suffolk, England, is one. This gentleman has made a variety of experiments, as he says in his book on the subject, on the colchicum autumnale, or meadow saffron. This has been generally supposed to be the root of the Colchicum Chinense floribus fritallariæ instar variegatis, foliis undulatis. His experiments, however, were uniformly inefficacious when the medicine was administered in small doses, and it was found most unmanageable in doses which he deemed moderate. He appeals to Mr. Want's own declaration concerning its occasional ill effects He was led to institute a variety of experiments with various articles, in the hope of discovering some safe and efficacious substitute for the colchicum, and Eau medicinale d'Husson, and after great expence and great labour he states he has at length succeeded in the invention of a *Tincture* with powers equally astonishing in removing the gout, as those attributed to the celebrated French medicine; and asserts that it has not in any *single instance* been attended with dangerous, or even violent effects. But the ingredient of his specific are kept *secret.*

Dr. Wilson lays his claims for the monopoly of the means of so much comfort and happiness to a most miserable part of the community, in the labour and expense in which its discovery has involved him. He appeals to many very respectable characters, among whom are a vicar and a baronet, as living witnesses of the utility of his Tincture in gout; and the gentleman who favored the editors with his book, stated that the remedy certainly had the effect of shortening the paroxysm. The Tincture agrees in some of its sensible qualities with the Eau medicinale, and the dose is about the same. Like it, it has been used by Dr. Wilson very successfully in acute rheumatism.

A Case of Demonomania, detailed by M. Bertholet, Médecin des Armees.—Jour. de Phys. February, 1515.

The subject was a female, and the occasion of the disease was a sermon delivered by a most violent fanatic. Bertholet saw this woman on the fourth day of the disease. He found her in the highest state of most deplorable mental excitement, nor was it till he assured her that he was thoroughly initiated in the secrets of hell, that she would give him any regard; but upon discovering this, she said that violent pain existed in her head. A bath was proposed: it was objected to, because it would increase the flame consuming her; it was however permitted afterwards, as it would satisfy him of her real condition by rendering the fire in her apparent. Extreme cold was applied to the head, by means of snow in a bladder. This was immediately followed by alleviation of symptoms, and its re-application was always attended with temporary good effect. On the fifth she was exorcised, after the ceremonials of the Catholic Church. Snow continued—its use remitted, violent delirium supervened—it was re-applied with benefit; purges were given, and opiates. A violent thunder clap at once operated a cure. The patient at once became calm, was wretched that she had behaved so ill, and gradually was restored to her usual health.

The following is from a memoir on partial Amputation of the Foot, by L. R. Villermé, D. M. P.—Journal de Physique, for February, 1815.

This is a very interesting memoir. It contains an account of Chopart's operation, first published in 1792, which has for its object preserving the posterior part of the foot, by amputating in such articulations of the tarsus as circumstances admit of. The operation was followed by partial re-union by the first intation, of the two flaps which were formed, to the articulating surfaces of the bone, and complete cicatrization occurred at the end of a month by suppuration of the rest of the wound. Chopart deserves much credit for this operation. A most important part is preserved by which, both standing and walking are secured to the subject of incurable ulceration of the other part of the feet, and which before this improvement had always been most strikingly impeded by the usual operation, viz. amputation of the lower leg.

Chopart tells us that so perfect was this treatment, that the patient stands in no need of mechanical assistance afterwards, but can walk perfectly well without it. This however was not supported by the experience of Villermé ; he says he was led to believe at the first examination, if we could add to the remaining extremity of the foot, another mechanical extremity, thus augmenting the base of support which has been diminished by the operation, we shall effect a cure of the disease for which we practised it, and in a great measure remedy the consequences of amputation ; for we thus replace in some measure, or form anew the portion of the dismembered extremity. This artificial extremity will have all the perfection of which it is susceptible, if maintained in the direction of the foot by a strong interior spring.

It will be impossible in this article more than to mention the advantages Mr. Villermé proposes as arguments in favor of the operation he recommends, or to point out in what it differs from that of Chopart. The author is astonished that the success of Chopart should not have led to new trials. It was not till 1812, that notice was taken of it by M. Richerand, in the *Nosographie Chirurgicale*, and in the Memoirs of Military Surgery by Mr. Larrey. The advantages proposed by Villermé appear to depend more particularly, 1st, on the remaining length of that portion of the foot which is anterior to its articulation with the leg, and on its size, which both better insure walking and prevent falling forward. 2d, On the facility we shall have, in applying an end of artificial foot, which will re-establish the whole extent of the base of support, and the whole length of the column which forms the inferior limb.

Announced Metallization of Charcoal.

Mr. Dobereiner, Professor of chemistry, of the university of Jena in Saxony, informs Mr. Accum, that he has discovered charcoal to be a metallic compound. The following lines he has received from Mr. Dobereiner on this subject: "I hope to be able to communicate to you the successful metallization of charcoal, which I have found contains a metallic base. In cast iron and in steel the metal is present in a metallic state, and may be separated from both of them by the united action of the phosphorus and an alkali. Further particulars concerning this subject I will transmit to you in my next.—Dobereiner." *Medical and Physical Journal, April*, 1815.

M. Donevan secretary to the Kirwanian Society in Dublin, has read a paper, in which he endeavours to shew that the principles of Galvanism and Electricity are different, the former being more immediately connected with chemical affinity than the latter.—*Ibid.*

In the same number of the London Medical and Physical Journal, from which the two preceding notices were taken, a case is detailed, containing an account of the good effects of the Tincture of Colchicum in gout. The author states that its good effects could not be attributed to its purgative qualities; since these first were produced without any alvine evacuation following its exhibition.

The author of the paper thought it important to communicate this case to the public, as Dr. Sutton, a late writer on gout, and Mr. Want, the discoverer of the Colchicum as a gout remedy, were at issue as to its *modus operandi*, the former attributing its good effects to the violent purgative effects of the tincture.

Congenital Hernia of the Heart.

M. Chaussier has presented to the Medical Society of Paris, the case of an infant who has a tumour at the superior and anterior part of the abdomen: it is soft, hemispherical, elevated about an inch from the surface of the body, two inches and a quarter broad, in which the alternate motions of the ventricles and auricles of the heart are evident. The appearance and size of the tumour, occupying a space from the anterior and lower part of the thorax to the navel, changes with the movements of respiration. When the child inspires, the heart ascends, and in part seems to return into the thorax. In *expiration* the heart descends, and its pulsations are more conspicuous. The tumour sensibly increases in size and tension when it cries, or when it is put in an erect position, and *vice versa.* The tumour diminishes when it is quiet, and the limbs are bent upon the trunk; a graduated pressure dissipated the tumour, but the respiration became more difficult; when the pressure was removed, the tumour returned. The child sucks, performs all its functions, and is likely to live.—*Ibid. January*, 1815.

A CURIOUS case is noticed in the same number, of a man whose skin naturally white, became black. This occurred in the Hospital la Charité, Paris. The man died of pulmonary disease, and on examination the discoloration was found to arise from a layer of black matter on the surface; the epidermis was found connected to it, and could not by any means be separated from it. Interrogated as to the probable cause of this discoloration, the patient ascribed it to vermin and uncleanliness; he had been lousy, and had worn the same shirt a long time; he never had the itch, venereal disease, or any cutaneous eruption.—*Bulletin de la Faculté de Medicine.*

THE Edinburgh Reviewers, by their unfair treatment of Mr. Abernethy, have given Dr. Adams an opportunity of explaining, in an eighteen-penny pamphlet, the supposed complicated doctrine of his favorite master on the vitality of the blood.—*Medical and Physical Journal, January*, 1815.

A NEW method of operating for the cure of Poplitial Aneurism has been employed in Dublin with the most complete success. The operation was performed by Mr. Crampton, surgeon general at the king's Military Infirmary near Dublin, 15th October, 1814. "The femoral artery was laid bare at the usual place, by an incision three inches in length and compressed, (but not tied circularly) by a narrow tape, so as completely to obstruct the current of the blood *with the least possible disturbance to the ar-*

tery, or *injury to its coats.* The ligature was, by a peculiar contrivance, applied in such a manner as to enable the operator to tighten or relax it at pleasure, without interfering with the wound. In two hours and a quarter the ligature was gently relaxed, but not completely loosened; no pulsation in the ham. In twenty-four hours the artery was relieved from all compression; but, as a measure of precaution the ligature was left in the wound. In forty-eight hours the ligature was withdrawn, and the wound was united by adhesive plaster."

The patient was examined on the fifth day by several professional gentlemen; his health was not in the least deranged. The tumour, which had decreased by one half, was without pulsation, and nearly incompressible. The temperature of each foot was 84°. On the fourteenth day the wound was nearly healed, and the man went about the ward on crutches. On the eighteenth day the wound was healed, and the tumour which could be seen only in the extended position of the limb, was incompressible and altogether free from pain.

The advantage of Mr. Crampton's operation consists in its imitating in the most favorable way the process of nature, in the spontaneous cure of aneurism.

1st. By interrupting the course of the blood through the ruptured artery, the fluid contents of the aneurismal sac are allowed to coagulate, and the circulation is thrown upon the collateral branches.

2d, The subsequent obliteration of the artery is effected by a natural process, which protects the patient from the long train of sufferings, and of dangers necessarily attendant upon the permanent contraction of a great artery, and its separation from the ligature by the process of sloughing or ulceration.—*London Medical and Physical Journal*, *February*, 1815.

Urine.

Professor Wurzer has published a set of experiments on a remarkable urine emitted by a man at the age of thirty-three. He had been afflicted with the gonorrhæa ever since the age of twenty-four. In consequence of cold, to which he had been exposed, his breasts swelled, and a few days after he passed the remarkable urine, which drew the attention of Professor Wurzer. It was milk white, and contained in it no inconsiderable quantity of a matter, which possessed exactly the properties of curd.—*Ibid.*

The Death of Dr. Warren.

THIS mournful event took place on the fourth day of April last. The name of this distinguished citizen and eminent physician, will go down to posterity with the history of our medical institutions, with each of which he maintained the most intimate and important relations. His fellow-citizens have given every testimony of respect for his character since his decease; but such testimonies may be given to office, to rank, and to fortuitous elevation. Let it be added then, that the community waited not for his death to award him honour. During every period of his active life, his fellow-citizens gave evidence, the most unequivocal, towards him of confidence, respect and love. Their high estimation of his character was acquired by the substantial services he rendered them; their affection and gratitude were won by his kindness and humanity.

The feelings of his contemporaries in regard to him cannot be transmitted to their posterity. Heaven directs that the best rewards of benevolence and fidelity shall be gathered in the days when they are exhibited. Talents may be acknowledged by posterity, but gratitude for personal services can be felt only by those of the passing age. Although, then, our descendants will know nothing of the tenderness of feeling, which has been manifested toward this distinguished man; yet they will know, and those of the medical profession particularly, much that he has done to promote the cause of science in our infant country. They will hear of him, as the father of our medical school, and as its first professor; as the President of our Medical Society during the most interesting period of its history; as the ardent and indefatigable promoter of medical instruction and knowledge. It is true, that the history of these public institutions will record much less than he really effected; for his services were never ostentatious. It will, however, record enough to preserve and consecrate his memory.

At the death of this excellent man the feelings of the community called for some signal mark of respect, some public manifestation of their sorrow. Immediate arrangements were made by the Counsellors of the Massachusetts Medical Society for solemnizing the funeral of their deceased President, and James Jackson, M. D. was appointed to pronounce an eulogy over the remains of the deceased. The measures thus adopted received the public approbation of the Rev. and Hon. President and Fellows of Harvard University. On this occasion a very crowded and respectable assembly gave evidence of feelings, the most honourable to themselves and to the memory of him, who was their object. The members of the American Academy of Arts and Sciences, those of the Humane Society and the Fraternity of Masons united in the funeral obsequies, with the members of the University and the fellows of the Medical Society.

On the Sunday following, occasional sermons were delivered in the church in Brattle-street, where the deceased had worshipped, by the Rev John Lathrop, D. D. and the Rev. Joseph M'Kean, D. D.; and in the chapel of the University at Cambridge, by the Rev. President.

On the 12th of June, at a quarterly meeting of the Grand Lodge of Massachusetts, of which both the deceased and his illustrious brother* had been grand masters, an occasional oration was pronounced by the R. W. and Hon. Josiah Bartlett, M. D. in presence of the supreme executive of the Commonwealth, and many characters of the highest standing in civil and literary life.

Died in Providence, (R. I.) April 1815, William Corlis Bowen, M. D. aged 30. In giving place to this record, some notice may very properly be taken of the character of the deceased.

Dr. Bowen had taken by his acquisitions in medical science, a place in the front rank of his profession. He possessed unusual deliberateness, and to this was joined great decisiveness. He was habitually reflective, yet his mind obeyed the demands of its objects, and rose to high conception of his professional duties, and these he successfully peformed. His profession had to him peculiar attractions; and when it allured him from his house and his country, it was only to reward him with the means of more extensive usefulness and a higher fame.

He distinguished himself abroad by the application of a strong mechanical genius, to the improvement of some of the instruments of his favourite art, viz.:—midwifery; and his zeal, as a dresser at Guy's Hospital, London, has been publicly mentioned by the gentleman under whom he served. The progress he was rapidly making to the highest medical standing, in his native country, was contemplated with sincere pleasure. He was but approaching the meridian of life, and yet he had discovered how bright that would be. The record of his character and attainments, nay, of his fame, however, is safe with all who knew him. It is left them to lament, that he was no longer permitted to bear it himself, in a continuance of his useful and honourable labours.

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* Gen. Joseph Warren.

List of New American Publications.

Remarks on Febrile Diseases, with a Definition of Fever; in a discourse, read before the Medical Society of Rhode-Island, at their third anniversary, September 1814. By Joseph Comstock, one of the censors of said society.

Sketches of Epidemic Diseases in the state of Vermont, from its first settlement to the year 1815, with a consideration of their causes, phenomena, and treatment, to which are added, Remarks on Pulmonary Consumption. By Joseph A. Gallup, M. D.

Observations on the Laws Governing the Communication of Contagious Diseases, and the Means of Arresting their Progress. Read before the Literary and Philosophical Society of New-York, on the the 9th of June, 1814. By David Hosack, M. D. F. L. S Professor of the Theory and Practice of Physic and Clinical Medicine in the University in the state of New-York, one of the Vice-Presidents of the Literary and Philosophical Society, &c.

An Introductory Discourse, delivered before the Literary and Philosophical Society of New-York, on the 4th of May, 1814. By De Wit Clinton, L. L. D.

Recent European Publications.

An illustration of Mr. Hunter's doctrine, particularly concerning the Life of the Blood, in answer to the Edinburgh Review of Mr. Abernethy's lectures. By Joseph Adams, M. D. author of observations in morbid poisons, 8vo.

Report of the Medical Effects of an Aluminous Chalybeate Spring, lately discovered at Sandrocks, Isle of Wight, pointing out its efficacy in the Walcheren and other diseases incident to soldiers who have served abroad, &c. &c. By William Lempriere, M. D. 8vo.

A treatise on Fever, with observations on the practice adopted for its Cure in the Fever Hospital and House of Recovery, in Dublin. By William Stocker, M. D.

Researches about Atmospheric Phenomena; by Thomas Foster, F. L. S. Second edition enlarged.

Practical Observations or Necrosis of the Tibia, illustrated with cases and a copperplate. By Thomas Whately. Esq.

A Treatise on the Diseases of the Arteries and Veins, containing the pathology and treatment of aneurisms and wounded arteries. By Joseph Hodgson, Esq. 8vo.

Engravings, intended to illustrate some diseases of the arteries. By Joseph Hodgson, Esq.

Mineralogical Nomenclature, alphabetically arranged, with synoptic tables of the chemical analysis of minerals. By Thomas Allan.

The Physiognomical System of Dr. Gall and Spurzheim, founded on an anatomical and physiological examination of the nervous system in general, and of the brain in particular, and indicating the dispositions and manifestations of the mind. By T. G. Spurzheim, M. D. Illustrated with nineteen copperplates, royal octavo.

Delineation of the Cutaneous Diseases, comprised in the classification of the late D. Willan. By T. Bateman, M. D. F. L S.

Observations on the Animal Economy. By a Physician.

TO CORRESPONDENTS.

A PAPER containing a case of laborious parturition, and monstrous birth, communicated by Dr. Tellfair, of North-Carolina, has been received. It was accidentally mislaid till too late for publication in the present number.

ERRATA.

Pages 248 and 250, for Depuytren, read Dupuytren. Page 256, for Barthey, read Barthez. These names were incorrectly printed in the English Journal, from which this paper was copied.

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A REMARKABLE EXTRA UTERINE CASE.

BY JOSIAH BARTLETT, M. D.

[Communicated for the New-England Journal of Medicine, &c.]

In October, 1807, I was called upon to attend Mrs. S. M. aged twenty, who stated that she had passed the common period of pregnancy with the usual symptoms, and that for several days she had felt what she was told were indications of labour. I visited her occasionally, in expectation of that event, two or three weeks, at which time her size lessened and her symptoms abated. Thus circumstanced, Dr. John Warren visited her in consultation, when it was agreed, to recommend patience, attend to the alimentary canal, and observe the operations of nature. All the appearances of parturition subsided, and she resumed the cares of her family. I frequently saw her until May, 1808, when a hard substance was distinctly felt on the right side of the abdomen, a little below the umbilicus, which frequently produced uneasy sensations, especially during menstruation, and continued till the operation which is described in the sequel.

In October, 1809, after a natural and easy labour, she was delivered of a healthy full grown child, and recovered without any uncommon symptoms. In November, 1812, the same event occurred, and she did as well as before. In February, 1815, she was again delivered of a healthy child, and for two weeks, was as comfortable as in her former confinements. In each of these labours, I inquired if she suffered any peculiar pain or inconvenience from the tumour in her side, and she always answered in the negative.

March 1*st*. About this period she became feverish, with a loss of appetite, a diminution of strength, frequent sickness at the stomach and occasional rigours, but had no cough. I visited her every second or third day until the middle of May, during which time, the child was put out to nurse; she took several doses of *ipecacuanha*, such mild laxatives as regulated the bowels, and various tonic medicines, of which *cinchona officinalis* was the principal, but without any material change in her situation. She then applied to Dr. Marshall Spring, to whom I had no opportunity to relate the case; but it was stated that he attributed her "great weakness and loss of flesh to the swelling in her side." He prescribed the tincture of *sanguinaria canadensis*, and *ipecacuanha* occasionally, as the most suitable emetic, and the powder of *cinchona officinalis* with conserve of roses, in preference to its being taken in wine, as it had been administered.

June 21*st*. She again applied to me, and stated, that she had gained nothing by changing the form of her medicines, that she was weaker and greatly emaciated, though she was better able to take animal food, wine, &c. than when I last visited her. The bowels were in a good state, but the tumour was more troublesome, and there was a small red spot near the navel. I advised her to suspend the use of medicine, for a few days, and to depend on nourishment.

24*th*. In the evening, a large quantity of matter, which was so offensive that the family was obliged to leave the room, was discharged from the tumour. On the following morning I made a particular examination; the discharge, which had been copious through the night, was more fœtid than any thing I had ever noticed; the opening was but just sufficient to admit a probe, and I distinctly felt bones, about an inch and a half from the surface. In the afternoon she was visited by Dr. Thomas Welsh, and the next morning by Dr. John C. Warren, both of whom confirmed my opinion of feeling the bones.

27*th*. A consultation was held with the above named gentlemen, Drs. David Townsend, Abraham R. Thompson, and William J. Walker, when it was unanimously agreed, that the only method by which the patient could obtain relief, would be an attempt to remove the remains of a fœtus, which we supposed was situated in a sack, formed by an enlargement of the right fallopian tube adhering to the peritonæum. I resorted to this hazardous and uncertain remedy in their presence, and that of three medical students, (Messrs. Gorham Bartlett, John C. Dalton, and Josiah S. Hurd) by enlarging the orifice, to introduce my left fore finger, which serving as a director, enabled me to dilate the wound, in the course of the linea alba, to such a size as to extract the child, the body of which had been full grown, and seemed to be doubled. The pelvis, (which was first extracted) spine and ribs, were not disconnected, and there remained a portion of the integuments and viscera, which were too rotten and offensive to dissect; the bones of the head followed in different portions, which with those of the extremities, were removed by repeatedly introducing my hand.* The hæmorrhage, during the operation, (which the patient bore with great fortitude) was small, and the wound was dressed superficially, without an attempt to unite its edges. Light nourishment was directed, she passed the day comfortably, and at evening took a dose of *tinctura opii.*

28*th.* She had a good night. The discharge from the wound, which was dressed twice, was copious, very offensive and tinged with blood. She had a small evacuation of urine in the forenoon, and was able to lay on both sides, which had not been the case for a long period. Her diet was light and nutritive; the *tinctura opii* was directed to be taken in the evening if necessary, with orders to administer a dose of calcined *magnesia* early in the morning.

29*th.* She took the *magnesia* about twelve o'clock, and passed a comfortable night without the anodyne. At six, A. M. I received information that the medicine had passed by the wound. On visiting her, I found that the discharge was greater, and as offensive as ever, and I feared that it was in part from the intestines. Directed a strong decoction, and the compound tincture of *cinchona officinalis* to be taken as freely as she could bear.

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* The bones are preserved, and will be deposited in the Medical Cabinet of Harvard University.

At three, P. M. there was a sudden and copious discharge from the wound, which was similar to that in the morning. Having renewed the dressings, I directed an injection; before evening she had four evacuations from the bowels, in which the *magnesia* was noticed, and she discharged urine freely, though accompanied with a bearing down, which she said resembled labour pains. She bore the medicine and nourishment, which consisted of wine whey, punch, broth, &c. and was directed to take an anodyne if she was restless. Her pulse and countenance greatly improved, she was moved without difficulty, and repeatedly stood on her feet.

30*th.* She took the anodyne in consequence of another discharge from the bowels, and passed a good night. The wound was dressed twice, the discharge as yesterday, but less in quantity; she had evacuations from the bowels and bladder, took the *cinchona*, wine, &c. regularly, ate moderately of animal food, and passed a good day.

July 1*st.* She took an anodyne the last evening; her bowels in good order. The matter from the wound, which was dressed twice, less in quantity and much better. At evening she sat up half an hour. The medicine, &c. continued.

2*d.* She had two evacuations from the bowels; the bearing down, at the discharge of urine, lessened; the appearance and discharge of the wound much improved; dressed morning and evening; she sat up twice this day, and continued the prescriptions.

3*d.* She took an anodyne last evening, and appeared rather more feeble than yesterday. The state of the wound improving, and the discharge much lessened. She had as usual two alvine evacuations, took animal food more freely, and increased the quantity of *cinchona* and wine.

4*th.* She passed the last night and this day very comfortably; the bowels in a good state; the pressing with the discharge of urine subsided; the digestion from the wound good, and it is gradually closing.

5*th.* The discharge from the wound was so small, and the appearance so favourable, that she required but one dressing. Her strength gaining rapidly under the treatment which was at first adopted, and she was able to set up two or three hours.

I continued to dress the wound daily until the 20th, when it was dressed every second or third day; the quantity of matter being very small. From this period the wound began to cicatrize, and at the date of this communication is so nearly healed, as only to discharge a small quantity of lymph. The patient has rode, visited her neighbours, and is in better health, than during the last eight years.

It should have been noticed, that I frequently injected warm water into the wound, and used alcohol freely over the dressings, which had a good effect in sweetening and strengthening the parts. The gentlemen who attended the operation, and others, called occasionally to notice the progress of the case, and I was much obliged by their attentions.

In the American Magazine, published at Boston in 1746, is related the history of a woman, with an extra uterine fœtus from 1731 to 1745, during which period she had six full grown children. In three weeks from her last parturition, there was an opening in the tumour near the navel, from which matter was discharged (about eight ounces in a day) for three months, when a portion of the head of a child was discovered, and some small bones were extracted; an incision was then made and the rest of the bones were removed at different periods, in four days, when the wound was stitched up. She died soon after, and on dissection, it was ascertained that the fœtus had laid in the left fallopian tube, which was greatly distended, and adhered to the peritonæum; the right tube and the uterus were in a healthy state.

In the Massachusetts Medical Communications, Vol. I. part III. page 30, is an interesting account of an extra uterine case, which continued more than seventeen years, during which time the woman had four children and several abortions. She died in 1802, when it appeared on examination, that the "uterus and right fallopian tube were in a sound state; the left tube was greatly enlarged, and from it was taken the bones of a full grown fœtus."

These, with the exception of one in 1783, published in the Memoirs of the American Academy of Arts and Sciences, Vol. I. page 551, where a quantity of hair was taken from an abscess in the abdomen, and a question arose relating to an extra uterine fœtus, are the only cases, in our Commonwealth, which have come to my knowledge, that bear any resemblance to the one I have now related.

It is remarked by a distinguished writer on obstetricks, that "in the records of medicine there is a very great number of examples of the extra uterine fœtus, in all of which there may be observed some similarity of circumstances;" that but "few practical remarks have been made on the subject, which can be useful to those who are in the way of meeting with cases of this kind. If an abscess should be formed in the side, or any part of the abdomen, and through the subsequent opening any part of the child should be evacuated, it will then be expedient to forward the exclusion of the remaining parts, either by enlarging the opening, or giving such other assistance, as surgery is very competent to afford."

It is further stated, that "when all the bones" of an extra uterine fœtus "are evacuated by the intestines or vagina, the parts affected gradually recover from the injury they have sustained, without any remaining mischief, and the patient usually enjoys as perfect health, as if no such accident had happened."

Having examined all the books in my possession, I can find but a single instance where a putrid fœtus was extracted from an abscess in the umbilical region, and the mother was preserved: this is related in Heister's Surgery, Vol. II. chap. CXIII. "on the cæsarean section."

Charlestown, Sept. 1*st.* 1815.

SPONTANEOUS HÆMORRHAGE.

BY WALTER CHANNING, M. D.

A NUMBER of cases of spontaneous hæmorrhage having occurred in the writer's practice, he was led to make the following inquiries and reflections on their probable sources and proximate cause. The cases consisted of hæmorrhage from the bladder in mild fever in one subject. From the rectum, in two cases. In these cases the loss of blood was very considerable, without any previous affection of the organ that was apparent, and in which a slight derangement only in some of the functions of the body had occurred a very few hours before the hæmorrhage. In three cases the hæmorrhage occurred from the calf of the leg. Of these, two were adult females, the third an adult male. In neither of them were the veins varicose, and in only one was there any ulceration, and in this the ulcerated surface was not much larger than that of a split pea. In one case the hæmorrhage occurred from the lungs under somewhat peculiar circumstances; one was a case of scurvy, and a number of them cases of epistaxis during measles. If there should be nothing new in any of the following inquiries and reflections, to any reader of the Journal, a principal object in their publication will be answered, if they lead to more elaborate and accurate investigations of the subject. The first inquiry is into the most probable sources of spontaneous hæmorrhages.

Among the many opinions which have been held on this point, two only will be mentioned. The first supposes the blood to flow from the mouths of those vessels which open into cavities, into secreting organs, into the receptacles of secretion, and into muscular and other structures; the great agents of supply and other important functions, constituting the exhalent and secretory systems. This doctrine of the sources of hæmorrhage has been designated by the term *anastomosis*. The second is termed *diapedesis* or *transudation*. This teaches that hæmorrhages proceed from pores with which the body is every where replete, and from those interstices which it is supposed may be formed, by accident or disease, by a greater than natural separation of the ultimate particles of which the solids are made, or, supposing their natural state to exist, in consequence of a greater than natural fluidity of the blood.

The first of these opinions appears most consonant with what we best know of the human body. For though disease may be regarded, in some measure, as an accidental occurrence or state of the system, it generally appears to us so modified by, or under the influence of what is natural or peculiar to the living body, as to permit, in some considerable degree, the usual phenomena of health; and although occasionally the natural functions of an organ may apparently cease in consequence of the violence of disease, still the diseased actions go on in the same vessels, in which the healthy ones resided; thus making these last ultimately the means,agents,or seats of curative processes. Farther, while we regard the various actions of the healthy body as the ultimate object of certain structures, we not only feel unwilling, but perceive no necessity to exist, to resort to other modes of explaining disease. While we retain what may be called at least a natural physiology, we seem in our investigation of disease to pursue a rational pathology.

The strongest arguments, however, for this opinion, are not drawn from its reasonableness; others more satisfactory may be derived from the condition of the general system, that of parts only, and peculiarities of the organs themselves under which hæmorrhages most commonly occur.

The greater number of cases which are mentioned in the beginning of this paper, are of hæmorrhage, which occurred among the other phenomena of disease. Some of them were unusual occurrences, others among the more usual symptoms of morbid action. The most striking were those hæmorrhages which took place from the bladder and lungs. The circumstances under which that occurred from the bladder were peculiar. The subject, an extremely delicate child, had been sick for some days, was extremely reduced by disease; an unusual flow of urine took place, which was soon followed by blood from the urinary organ. This case goes far in support of the opinion adopted in this paper. The general state of the system predisposed the organs in general to take on diseased action. The functions of the kidneys became principally disturbed. So greatly was their secreting power increased in the first place, as to produce a very unusual quantity of urine. The condition, whatever it was, of the vessels or the organs subsequent to this action, permitted the escape of the blood along with the natural secretion. The affection, most probably, did not amount to inflammation; hence no pain was complained of, or it was so low a degree of inflammation as only to be followed, first, by increased urine and then blood.

Farther, the *rare occurrence* of hæmorrhage from organs labouring more or less under disease, is not an argument against the doctrine of anastomosis; but, were the notion of transudation true, we might be rationally surprised that we do not more often find effusion of blood into the various cavities of the body. In this last case the effusion would be merely an accidental, not an unusual occurrence. In the first, on the contrary, it can only occur as a consequence of the living agency, and be among the anomalous occurrences of diseased function.

If the remarks just made be true, their application to the more common cases of hæmorrhages may be extended without violence, to those cases also to which the doctrine of transudation would appear to have a more legitimate reference. I now refer particularly to those which appear under circumstances of peculiar malignity, which, from their phenomena, have received the appellation of putrid; and in which, not only the solids, but the circulating fluids have undergone certain changes, in consequence of which some of their distinctive living characters, and more especially those of the last, appear in a measure lost.

But if blood of its usual qualities can and does at times pass extremely minute vessels, vessels which have very peculiar offices in the system to perform, and originally were never designed to give passage to the blood, *a fortiori*, how much more readily will the blood find a passage through the same vessels, attenuated as it is by disease, and the vessels themselves, in their want of function or action presenting but a slight obstacle to its escape?

Again, it has been asserted by those who support the opinion of transudation, that were hæmorrhage truly the consequence of anastomosis, we should very often find it occur in those higher actions of which those vessels are susceptible and so often suffer; for instance, during a state of inflammation. And the argument is supported by the increased secretion which inflamed organs at times afford.

Now it is a well known fact, that inflammation is at times attended with increased secretion, and sometimes even by effusions of blood from the affected organ. But as inflammation is a process totally unlike the natural, or rather healthy functions of a part or organ, attended by its very nature with very extraordinary, but fixed, peculiar, I might say natural phenomena, we should almost, nay quite as soon, look for hæmorrhage during the most healthy state and functions of an organ, as during the occurrences of genuine inflammation. During genuine inflammation, the vessels are called upon to do something more than in health; they are not merely to preserve the integrity of the organ to a degree consistent with its vitality, but in taking on themselves new functions, to restore to it health. All that is designed in these remarks, is to show that because the secretions of an organ may be increased by inflammation, or what appears to be genuine inflammation, and because hæmorrhage is not always among its phenomena, we are not hence to infer that its sources are not vessels, and its occurrence not among the actions of which those vessels are susceptible.

If we advert for a moment to diseases in which hæmorrhages are most frequently found to occur, we shall find them diseases of organs remarkably vascular, and perpetually performing very important secretory functions. Thus we find the diseases of the mucous membranes most frequently accompanied with hæmorrhages, and next to these, those of the serous membranes.

Dysentery, that distressing affection of the mucous membrane of the lower portion of the intestinal canal, furnishes us with a striking illustration of these remarks. All the facts connected with its peculiar phenomena, viz. the discharges which characterize it, lead us to the vascular origin of its occasionally accompanying hæmorrhage. We have, for instance, in the first place, the increased mucus covering the fæces; then pure mucus, then mucus slightly tinged with blood, and at last the simple coagulum of blood itself, or that coagulum combined with portions of lymph which the motion of the intestines, their pressure or other causes have detached from the common mass, or which the small vessels have immediately yielded; and sometimes a small portion of mucus, that the lowest vessels of the rectum may be still able to secrete.

When we examine the body after death from this disease, we discover, not an abraded surface of mucous membrane covered with pus, but a surface smeared with a bloody mucus, or covered with a layer of lymph, similar to the discharges during life; and when we have removed these, we sometimes observe the turgid secreting vessels, constituting those dots which authors have mentioned; at other times other appearances of inflammation; while sometimes we only observe marks of organic disease so slight, as to be scarcely visible. The phenomena just particularized may all be satisfactorily explained without referring any of them, either to the abrasion to which an ulcerative process might be liable under the existing circumstances of the disease, or to that partial disorganization which the violence of the disease might be conjectured to induce, and which has been considered by some so favourable for hæmorrhage by transudation.

If spontaneous hæmorrhages have their source in vessels, the next subject for inquiry is their proximate cause, or that state of vessels of which they are the effect. To answer this question, there are far greater difficulties to surmount, than were met with in pursuing the first inquiry, and, at best, we can arrive at but a rational hypothesis. In the greater hæmorrhages which occur from secreting organs, properly so called, we find in the first place a partial temporary suspension of the secreting function, though, at times, the hæmorrhage is preceded by an evident increase of the secretion. The vessel, in other cases, which heretofore delayed the blood,(if I may so say,)that it might select from that fluid what was most appropriate to the performance of its function in the economy, or that it might from the whole mass elaborate its secretion, (which, by its healthy structure and during its healthy functions, could not admit the passage of the fluid in its entire state,) is now found to give the blood a free course. The peculiar irritability with which the vessel was endowed,and which preserved,while it constituted its healthy tone, is found to a degree wanting, and the stimulus of what may be called a foreign body is not perceived by it. If we examine the purely exhalent system, from which, at times, hæmorrhages proceed, we also find, that that has undergone great changes from its natural state and function. Instead of separating from the common circulating mass, that portion of it which will answer its peculiar purposes in the economy, and the just quantity of which is so exactly preserved by the balance between exhalation and absorption, we find the blood itself effused ; at times, in such quantities, merely as to produce but slight inconvenience, at others, fatal effects ; its consequences always depending on the circumstances inducing or attending the discharge, and on the cavity into which it is made. Let us now inquire, whether the state of the vessels, favourable to hæmorrhage, be secondary to a peculiar condition of the general system—in fact, be produced by it,and simply a symptomatic affection ? Or is hæmorrhage the consequence or symptom of a primary, idiopathic affection of the small vessels,of which the peculiar state of the circulation,and the morbid condition of the whole system are but symptomatic?

This inquiry is, perhaps, of more importance than at the first sight it may appear. Hæmorrhage is certainly among the alarming phenomena of disease, and at times, and with the uninformed always among the most alarming that occur. It is, however a fact, that it is by no means always among the most dangerous phenomena of disease, and at times it appears to us under the character, and certainly, with the effect of a curative process.

To the above questions, it will be perfectly safe and perfectly true to answer, that hæmorrhage undoubtedly occurs as a consequence of an original general disturbance in the system, and is thus among its symptoms; and is also, in other cases, the effect of an original affection of those vessels from which it proceeds. The vessels from which it may proceed, are remarkably under the influence of the various causes which affect the body, and particularly the more powerful ones, and especially during much local or general debility. Thus we find them, at times, influenced by the state of the mind. A patient, ill of pulmonary disease, requested a consultation. In this case, hæmoptysis had never occurred. The disease, though of long standing, had not apparently produced great disorder in the lungs. It seemed chiefly confined to the mucous membrane. She was a little agitated by the presence of her new physician, which agitation, however, soon subsided, and as she was coughing, as she usually did, the physician begged her to permit him to see the matter expectorated. It was found to be chiefly blood. It produced the alarm and distress that such an occurrence might be supposed to excite; they soon subsided, and in a few hours, with very simple remedies, the hæmorrhage ceased, and has not again appeared. The disease has not essentially altered. In this case a very sudden change was effected in the state and functions of the small vessels. It was one of the ultimate effects of some operation of the mind. The small vessels were suffering a debility induced by protracted, though slight organic disease of the lungs, and in which they were most intimately concerned. The circulating system also possessing a morbid mobility, was suddenly affected by the mind. An unusual quantity of blood was thrown on the small vessels, and they readily gave it a passage through and from them. In this case, although the hæmorrhage depended on the altered state of the circulating system as its exciting cause, it was, nevertheless, in part, the effect of the previous condition of the small vessels, viz. of their predisposing debility. A case is related in the

Parisian Journal of Medicine for January, 1815, edited by Sédillot, in which the hæmorrhage occurred from the skin; the blood freely passing from the vessels of perspiration. This remarkable occurrence, was among the phenomena of *nephritic colic*, which disease was directly and immediately produced by a violent fit of anger. The small vessels appear, in the case of this woman, to have been remarkably susceptible of the action on which hæmorrhages depend, and which was produced by moral causes. During a fit of anger, two years previous to the bloody perspiration, considerable hæmoptysis and cough occurred; and this was always the consequence of mental disturbance, unless peculiar nervous symptoms, to which she was also liable, were produced by the fit of anger. The *last* paroxysm of the colic she suffered, took place without manifest cause; and the hæmorrhage was, in this instance, confined to the skin of the face, neck, armpits, and anterior part of the thorax and abdomen. At the first view of this case, the bloody perspiration which occurred among its phenomena, may be considered a sympathetic affection, the original one being the nephritic colic. With more correctness, however, may it not be considered as the direct consequence of that state of mind which induced the colic; and this, notwithstanding its occurrence during a paroxysm of colic, which was not the consequence of the original cause, viz. the mental disturbance? The hæmorrhage had occurred repeatedly with the colic, and thus they had become intimately associated, not as cause and effect however, but as the effects of any cause which might be sufficient to the production of either of them. The small vessels of the skin and those of certain abdominal viscera, were precisely in the same state of predisposition to any morbid impression. The varied and remarkable effects of such impression, however, depended on circumstances peculiarly calculated to admit of their production. The hæmorrhages, in these cases, were the consequence of derangement in the whole system, were in short secondary affections in the small vessels. In leaving these, to consider those which proceed directly from the morbid actions of the small vessels themselves, we enter the most interesting and widely extended field of pathology.

What very important morbid action, what disease can we advert to, in which the small vessels are not most intimately concerned? Dysentery, which we have before adverted, what is it but a disturbance in the functions of the small vessels of the intestinal tube, most intimately associated with a similar one of those of the skin? It is true, that in the milder cases of this disease, hæmorrhage may not appear. This is very easily explained, without yielding the position that they are the seats of hæmorrhage when that does occur. They are able, by their great and easy adaptation to new occurrences in the system, to dispose of, or relieve themselves of the great afflux of blood to them, by exerting themselves in an extraordinary degree. But when the impression of the exciting causes of the disease has been very violent, when especially the predisposition has been perfectly formed; after a few days of violent secretion, of violent effort to effect a cure, if I may so say, their natural healthy functions in a great degree cease, and pure blood passes unimpeded into the intestinal canal. In this case, then, hæmorrhage legitimately belongs to the small vessels as the immediate effect of their own actions. If these are not soon suspended by those means which diminish morbid irritability in them, as in all other organs of similar properties, they communicate to the system at large, or excite in it, and directly too, disturbances which advance with more or less rapidity to a fatal termination. I might mention here the fatal convulsions which have occurred in dysentery, in children, this season in this place.

Take another disease, an inflammation, for instance, of the lungs. In this affection, notwithstanding the general disturbance which exists in the system, the rapid and strong pulsation of the heart and great vessels, the heat, &c. which might lead us to trace the local affection entirely to that of the general system, the small vessels of the diseased organ, may perhaps more correctly be considered as primarily affected, and as suffering disease from their intimate association with those of the surface; and in the advanced stages of the disease, when hæmorrhage becomes its accompanying phenomenon, they are not only the great agents of the hæmorrhage, but to their diseased functions are to be then traced the various morbid affections which are exhibited in the general system. That this reasoning is true, we have only to advert to the treatment of this disease, which consists of two great objects, viz. to diminish the action of the small vessels; and then to induce new actions, which on the principle of mere change effects the cure; or, by suspending disease, leaves the organ at liberty to resume its healthy operations.

There is nothing more common in our pathological observations, than to view diseases in the relations of cause and effect. It has been an object in this paper to consider diseases, which appear to have these relations, not exactly in this light, but to argue that the predisposition to disease in any one organ may not have been confined to that alone, but has been extended to other organs in its neighbourhood; and that the operation of the exciting causes on each, in the production of the disease, has appeared sooner or later in one or the other, in consequence of peculiar circumstances of structure or function, or of susceptibility to the action of those exciting causes; not in consequence of the mere operation of sympathy.* To illustrate this reasoning, I would advert to some very remarkable cases of hæmorrhage constituted by a vicarious performance of a certain uterine function, viz. the catamenia. Thus the catamenia instead of appearing from the cavity of the uterus, have been secreted into the cavity of the abdomen; or in place of uterine hæmorrhage at the proper period, hæmorrhage has occurred from the serous vessels of the peritoneum. Now, in this case, the predisposing causes of obstruction affected the small vessels in the neighbourhood of the uterus; and the exciting causes found the first more susceptible of their influence than those of the last, or, what is more probable, the exciting causes of the obstructions directly affected the small vessels; and the derangement of the general system was more readily relieved through them, than through the secreting function of the uterus.

These remarks have of course a limited extent. They are meant to support the opinion of the primary affection of the small vessels in the species of hæmorrhage under consideration.

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* It may be replied here, that even under these circumstances of extended and universal predisposition, the fact of a progression in diseases proves the doctrine of sympathy. All that is meant in the above remark is, that the second organ is not affected with disease merely in consequence of the disease of the first, but that the order of occurrence is the consequence of circumstances altogether foreign from sympathy.

It would be interesting to extend these remarks to various other species of spontaneous hæmorrhage. Scurvy offers us an instance in which this phenomenon is very generally to be observed. The proximate cause of hæmorrhage in this case is strictly an idiopathic affection of the small vessels. The peculiar condition of these vessels on which the hæmorrhagic action depends is very extensive in this disease. For though we find the actual escape of blood from the body occurring sensibly from the gums, we also find a strong tendency to the same phenomenon in every part of the body which comes under our notice. We observe it particularly in the skin. Now although there be much in the cuticular spots, which are observed in this disease, which resembles some of the exanthemata both in their access, progress, and decline, perhaps they would with more correctness be said to owe their irregular appearance to the various degrees of predisposition the small vessels possess, to an effusion into the skin; that in short, such an universal debility in them as would admit an universal and simultaneous escape of blood into the skin, and cellular structures, would amount to positive death. We are to consider the gums and small vessels of the skin, in common with the whole system, under the influence of the predisposing and exciting causes of scurvy, that the hæmorrhage in fact is but a symptom of the idiopathic affection of these vessels.

A classification of hæmorrhages has suggested itself in the course of the preceding inquiries, which, it was thought, would render their investigation more easy, and the communication of what might be discovered, far more perfect, than that which is founded on their remote or exciting causes. This latter method has been adopted in the divisions of M. Lordat, who published a work a few years ago, expressly on the subject of hæmorrhages. The classification suggested, is to be founded on the *textures*, or organs from which most generally the hæmorrhages proceed. Thus hæmorrhages from the mucous membrane should constitute one class or genus; those from the serous another; the muscular organs a third, &c. &c. while the species should be founded on prominent circumstances more immediately concerned in the actual flow of blood.

The writer is aware that this method is condemned by M. Lordat. But this judgment of this highly respectable author, he is also aware was the consequence of the peculiar notions of that author, concerning the share the small vessels of the various textures or organs of the living body have, in the production of hæmorrhage, viz. that they are but secondarily concerned in its production.

Something may have been expected in the course of this paper relative to the modes of treatment of hæmorhages. The limits prescribed by the nature of the work in which the paper appears, have prevented an investigation into the modes of treatment. In fact, the treatment of hæmorrhages resolves itself either into the treatment of those diseases in which they appear, or which they constitute; or when it is but a purely symptomatic and sudden affection, the consequence of derangement of the whole system, into the treatment, or means of relief of the general system, with whose disorder it is associated, or of which it is but a symptom.

Hæmorrhages, as they have been considered in the preceding pages, are very often the results of disease in the small vessels. These vessels are intimately connected with the whole circulating system. This last of course is always more or less affected by the diseases under notice, and in a majority of their diseases it is believed that the best means of cure may be found in direct depletion from the circulating system. The writer has seen with pleasure the practice of bleeding becoming more and more extensive in this part of the country; and on unquestionable authority would mention the success of bleeding in some cases of our dysenteries.

Hæmorrhages on the other hand depending on other circumstances will require their appropriate treatment. Those which are produced by sudden commotions of mind or body, or both, will at times be best treated by bleeding, at others by narcotics or antispasmodics; others the consequence of peculiar courses of diet will be best treated by entire change in the food, and by tonics. Scurvy furnishes us with instances of this kind; while critical discharges of blood will cease with the other circumstances attending crisis. Febrile diseases attended with petechiæ, and discharge of blood from various organs, of course, will demand appropriate treatment.

The question of the precise nature of the action of the small vessels, on which hæmorrhage depends, although involved in the preceding inquiries, has been carefully avoided. The motives for this silence, the writer confesses to have been others, and those more powerful, than the length to which the paper has already extended. This last motive has however had some influence with him in bringing it to a close.

REPORT OF A CASE OF INGUINAL ANEURISM.

BY J. B. WHITRIDGE, M. D. *of the United States Army.*

WILLIAM TROBRIDGE, aged nineteen years, born in the town of Norwalk in Connecticut, of a robust constitution, and by profession a shoemaker, enlisted into the third regiment of artillery at New-York, in August, 1812, from which time he served as a soldier, (principally on the frontier) and generally enjoyed good health, until the 7th August, 1814, in attempting to pass the laboratory guard at Sacket's Harbour, (at nine o'clock in the evening) was shot by a sentinel on post. From the best information I could obtain of the attending surgeon, and from dissection, a buck shot passed through the dorsum penis, entered the right groin, penetrated the inguinal glands, passed nearly in the direction of the femoral artery, over the os femoris, and lodged in the vastus externus muscle. At the time the wound was received, he is represented to have bled profusely, even to deliquium animi; this however soon subsided: he was taken to the Hospital, and superficially dressed, no hæmorrhagy followed. The wound was treated in the usual manner for gun shot wounds, and after a lapse of about two months, on the 16th of October, was discharged from the Hospital, and reported for duty. He performed the ordinary duties of a soldier, until about the middle of December, when he states that he was taken suddenly on post, with a pain in his thigh; he reported his situation to the officer of the guard, and was relieved by another sentinel. His thigh was very painful, and on examination, he found it much swollen. He suffered the pain and swelling to continue a day or two, and then made application to me. He represented it as a recent difficulty, but observed, (to use his own expression) that he thought his thigh was going to break out again.

On examination, I noticed his whole thigh to be much tumefied, from his groin almost to his knee, but most about the groin, and particularly at the part where the shot entered. I pressed it on every part with my hand, but could feel no pulsation. The patient being interrogated on this point, observed, that he had felt none at any time, though there was some heat about the tumour, the temperature of which was somewhat above the rest of the body; as there was no redness at the surface, I thought the case a very singular one. The tumefaction being represented as a *recent difficulty*, I thought it not improbable, as the pulsation was wanting, which is ever considered an inseparable diognostic, consequently an infallible criterion of aneurism.* I supposed it the incipient stage of a deep seated abscess, consequent to the wound as its remote cause, perhaps resulting from a wound of the os femoris by the passage of the buck shot subjacent to the extensor muscles (that this was not the case was a circumstance at that time unknown to me).

As the tumour seemed rather of an indolent nature, I thought it best to attempt if possible to discuss it; to this end I directed an epispastick, of meloes vesicatorii, to be applied; a cathartic of sulph. sodæ to be given him; and ordered him to return to his quarters.

From the pressure of business, I was unable to pay that minute and particular attention, which his case merited. I did not see him again until the second day following, at which time he came to me and reported his situation unrelieved—his disease unabated. The blister it appeared had done no good, but on the contrary, had probably done injury—this however the patient was not sensible of; at any rate, reasoning *à posteriori*, it is rational to conclude that if it did not answer the purpose for which it was intended, in as much as it had any action, the effect must have been injurious.

I considered the case an important one, and as he could not receive the necessary attention, nor have the necessary accommodation in the barracks, I ordered him to the Hospital on the 22d of December. For the first day or two after his admission into the Hospital I prescribed nothing for him, but waited to see what course his disease would take. The swelling increased, and the integuments became very tense. I supposed suppuration would take place, and with a view to promote this end, ordered emolient cataplasms to be applied; these were continued five or six days without intermission.

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* Except indeed a case of axillary aneurism described by Mr. Pelletan, and perhaps some others.

The effect was, to relax the integuments, to remove the tension of the surface, and to abate the swelling. A fluctuation became in some measure perceptible, though so imperfect as to leave the mind in doubt as to the nature of the tumour, in as much as it did not point at any place, in the usual manner of abscesses, which contain pus. The integuments, however, were thinner at some places than others, and most so at the point where the shot entered; which led to the opinion, that if the abscess contained pus, and were left to itself, it would burst *at that point.* The cataplasms were discontinued after five or six days application, in consequence of their becoming painful, and disagreeable to the patient—having done all the good that they would, or that was expected. And I think, it is an established fact, that poultices to suppurating wounds or abscesses, or those disposed to suppurate, do no good, after a certain period. A longer continuation of them does material injury. This period must be determined in every case, by the experience and judgment of the practitioner. Although the poultices were discontinued, no pointing had taken place. I applied a flannel roller about the part, and waited five or six days for this event, with an intent to open into the morbid cavity, as soon as the integuments should become thin, and the abscess should shew a disposition to evacuate its contents. While deliberating on this, instead of exhibiting any disposition to open, the whole thigh again became swelled and painful; some heat ensued, the parts became rigid, and the fluctuation less perceptible. I ordered the cataplasms to be re-applied, and to be changed every three hours—this was promptly done. They were continued a day or two, producing the same effect as before, and were again discontinued for the same reason. In consequence of the flaccidity of the parts, the fluctuation, though indistinct, now became quite perceptible. The disease appeared so extraordinary in its nature, that I still hesitated to lay open the tumour, doubting what the contents might be, as I was ignorant of the cause, and therefore fearing the consequences of an incision. Whether it was the effect of a rupture of an artery, vein or lymphatic, or whether it was an indolent suppurating abscess of extraordinary magnitude, was uncertain; the latter seemed most probable.

The patient constantly lay upon his back, and kept his leg in a state of moderate flexion upon the pelvis, and also at the knee. After the cataplasms were laid aside, I again applied the flannel roller, in order to keep the limb warm; and to prevent the protrusion of the matter upward under Poupart's ligament, and backward among the glutei muscles, I applied it firmly in the groin above the tumour. The health of my patient, during this time continued tolerably good, his appetite however was poor, and his countenance pallid, in consequence of the pain of his thigh and of being confined to his bed.

Several days elapsed before any thing was done; the temporary good effects of the cataplasms did not long continue; his thigh again began to swell, and very soon attained to an enormous size. During all this time, no pulsation was perceivable, neither could the tumour be made to recede upon pressure. Although an incision had been proposed, I felt delicate about attempting it, until I had called a consultation; a thing I am sorry to say rather unusual in the army, for want of talents and education, and that harmony between my medical brethren, and that confidence in each other, which is so necessary for the advancement of medical science.

Owing to some untoward circumstances, the consultation did not take place until the 8th of January, at which time, the integuments had become very thin, much distended, very painful, and almost ready to burst.

After witnessing a military execution, I then took with me, Dr. Thomas Harris, surgeon of the United States navy, an amiable, fine young man, a scientifick physician, and a well read surgeon. He had previously heard the history of the *case*, and on examination expressed, and appeared to feel much doubt, as to the nature of it; but concurred with me in the opinion, that the tumour most probably contained pus, and recommended an incision into it, at the thinnest part of its parietes; which was at the point where the shot entered, directly over the crural artery. To this I objected, (in as much as we both felt a degree of uncertainty relative to the nature of the tumour) lest an incision at that part, might jeopardize the life of the patient, in case it should prove to be a disease of the artery. It was however determined to lay it open, upon the external part of the thigh, to afford an opportunity for the morbid cavity, if it contained pus, to evacuate itself; if not, the better to obviate the consequences of the incision.

We were so far persuaded that pus would be found, on laying open the cavity, that we were unprovided with other instruments than those necessary for this operation. I made an incision into the tumour, through its parietes, three inches and a half in length, and about four fifths of an inch in depth. Coagulated blood and pus were immediately evacuated, but not freely. I extended the incision upwards of two inches farther, it then not freely evacuating, I commenced clearing it with my thumb and fingers, aided by a pair of common long forceps.

Large portions of very firm coagula were removed, covered with a small quantity of pus on that part which lay contiguous to the parietes of the tumour.

After having removed at least one quart of its black and fœtid contents, a sudden and tremendous gush of fresh arterial blood announced the nature of the disease. I immediately seized the limb, pressed the two edges of the incision firmly together, and held them in that situation until I could command the flow of blood by other means. No tourniquet being at hand, I called for a handkerchief, which my assistant, Dr. Harris, applied firmly in the groin, upon the upper part of the tumour, and twisted it up with a stick, which was held in that situation, whilst I laid a large compress of patent lint upon the opening which had been made, and applied a roller about the thigh. The hæmorrhagy being now completely restrained, I had an opportunity for a few moments deliberation.

The disease was evidently an aneurism of the diffused kind; but whether it was a false aneurism, agreeable to Scarpa's doctrine of all aneurisms, and according to Mr. Home and Dr. Hunter in some cases, or a true aneurism agreeable to the definition of most writers on this subject, was uncertain, and altogether immaterial, as the treatment is the same.

The sudden loss of blood occasioned a partial syncope, a very great depression of the pulse, and prostration of strength, together with a pale and cadaverous countenance with rolling of the eyes.

These symptoms were accompanied by a very considerable degree of mental derangement, occasioned by the sudden abstraction of stimuli, which seemed to be a delirium from inanition.

His pulse became almost imperceptible, he fell into a cold sweat, and in short, his general symptoms were very alarming and even threatened immediate dissolution, although the quantity of florid arterial blood which he had lost, perhaps did not exceed half a pint. Stimulants, such as wine, brandy and water, &c. were given him, which had the effect to raise his pulse a little, and in some measure to restore him, although it did not relieve the delirium. It became necessary immediately to resort to an operation, as the only alternative, to preserve the life of the patient, which could be attempted with very little hope of success.

After considering the case, it was finally agreed, as a dernier resort, to take up the external iliac artery as high as possible above Poupart's ligament. Accordingly the necessary preparations were made. I was unprovided with an aneurismal needle of any kind; and for want of the needle invented by Dr. Physick, for securing bleeding arteries in deep narrow wounds, I took a common surgeon's needle of the largest size, broke off the point of it, reduced its temper by heat, made the end of it smooth, gave it such a curvature as I wished, and adapted it to a pair of common straight forceps, which I substituted for Physick's curved forceps, and tied the handles.

Every thing being now ready, the patient having previously taken a pill of camphor and opium was placed upon a table of convenient height, and the operation performed on Sunday, January 8th, at 5 o'clock in the evening by candle light; in presence of Dr. Harris and Dr. Gordon of the navy, and a number of gentlemen of the army.

Without entering into a minute detail of the manner of operating, suffice it to say, that the operation was performed in a manner similar to that described by Dr. Dorsey, which was the first and only case in America, that I have seen reported, in which this operation has been performed. The principal difference of this operation was in the situation and extent of the incision. The situation of the artery, of the spermatick cord and of Poupart's ligament, being previously marked out, I commenced my external incision at a point, upon a line, from the anterior and superior spinous process of the os ilium to the umbilicus, about two inches from the former, and four from the latter. This incision was extended obliquely downward, five inches, dividing the skin and adipose membrane, and terminated within one inch of the symphysis pubis. By the subsequent incisions, the tendon of the external oblique muscle, the internal oblique, and the transversalis abdominis were divided, but the incisions were not extended so low, as to wound the epigastric artery, or the spermatic cord. I met with considerable embarrassment in this operation, from the delirious state of my patient, who kept constantly hallooing, making strong inspirations and expirations, writhing and contorting his body, throwing the abdominal muscles into violent action. But the greatest difficulty I met with, was what Mr. Freer so much complains of, that of passing the needle round the artery. This I was unable to do, from the great quantity of adipose substance, with which the artery was enveloped, until I had denuded the artery by careful dissection, and a division of the fascia. This part of the operation was the more dangerous and difficult, in as much as I was under the necessity of performing it by candle light.

The strength of the patient was preserved during the operation, by the repeated administration of stimulants; and after it was completed, the handkerchief and the bandage were removed from the thigh; that part of the coagula which protruded immediately at the orifice of the sack was also removed, the limb washed, and superficially dressed, by the application of a large pledget of lint, and a roller. The patient was then placed in bed, with his leg in a relaxed position, supported by pillows.

The patient though somewhat restless, passed the first night as comfortably as could be expected under existing circumstances. The relative heat of the lower extremities was not observed previous to the operation. The thermometrical temperature after the operation, at twelve o'clock in the evening, when I made the experiment, was as follows.

Temperature of the room	- - -	66°
do	under the tongue - -	97°⅓
do	of the right ankle, or the ankle of the aneurismal limb - -	86°
do	of the left ankle -	88°½

Monday, January 9th. At five o'clock and twenty minutes, A. M. examined the temperature between the toes, and repeated the experiment at the ankles.

Temperature	between the toes of the right foot	85°
do	do left foot	94°
do	of the right ankle	90°⅓
do	of the left ankle	90°

Temperature of the room being about the same as before.

At eight o'clock my patient was perfectly calm and quiet, but recollected nothing that had passed since the laying open of his thigh. The blood was constantly oozing from the orifice of the tumour, which kept the dressings and cloths which were laid under it quite wet. At twelve o'clock I took off the dressings from the thigh, (but left those about the body undisturbed) and removed that portion of coagula which lay contiguous to the incision. At twenty minutes past three o'clock, P. M.

Temperature	of the room	66°
do	under the tongue	100°
do	aneurismal limb at the ankle	92°
do	left leg at the ankle	93°

No considerable variation in the evening.

He remained very quiet through the day, but felt no disposition to sleep at evening. At ten o'clock, I gave him half a grain of opium, which had but little effect; at two o'clock, a pill containing one grain and a half of camph. and half a grain of opium was given him, which procured some sleep: as he had had no evacuations from the bowels since the operation, to prevent the constipating effect of the opium, at four o'clock, I directed half an ounce of ol. ricini to be given him.

Tuesday, January 10th. Some difficulty of respiration, pulse full and hard, heat somewhat increased, ordered sub. muriatis hydr. gr. v. rhei. gr. x. which produced several dejections in the course of the day, without much alleviation of the symptoms. In the evening there was quite an increase of heat, which was as follows:

At 11 o'clock in the evening, temperature of the room		67°
Temperature	under the tongue	100°
do	right leg above the knee	96°
do	left do do	97°
do	between the toes of right foot	96°½
do	do left foot	98°

The thigh was dressed as yesterday, and at least one pint of coagulated blood removed, by my thumb and fingers, and by pressure upon the thigh. At twelve o'clock at night, bled him oz. 14, which gave him some relief, but occasioned no sleep; at two, he took a pill of one grain and a half of camphor and half a grain of opium, after which he dozed until morning.

Wednesday, January 11*th.* Morning.—Patient somewhat better, complains of much hoarseness, and a dry throat; directed syr. scill. marat.

At half past two o'clock again examined the heat of the body.

Temperature of the room being		63°
do	right leg above the knee	97°
do	left do do	98°
do	between the toes of the right foot	94°
do	do left foot	95°

Arterial action something above the healthy standard, complains of some tightness across the thorax; having no alvine dejection, at eleven o'clock in the evening, gave him gr. iii. calomel and gr. vi. of rheum. He has complained of considerable thirst for two or three days; his principal drink has been lemonade; food, water gruel, milk porridge, crust coffee, &c.

Thursday, January 12*th.* The cathartic of the preceding evening not operating, at twelve o'clock, ordered half an ounce of ol. ricini which produced four or five evacuations, and a considerable reduction of the pulse. The temperature of the extremities was also reduced two degrees lower than yesterday.

Friday, January 13*th.* Condition of the patient much the same as yesterday; flattering prospect of recovery. At 10 o'clock in the evening, after dressing:

Temperature above each knee		94°
do	between the toes of the right foot	94°
do	do do left do	96°

At 11 o'clock directed tincturæ opii. gutt. xv.

Saturday, January 14*th.* The anodyne, not having the desired effect, at 10 o'clock in the morning, ordered a repetition of the dose, and, at six, an aperient of ol. ricini; syr. scillæ mara. continued. An anodyne in the evening. The aneurismal sack which has now become a complete abscess, has been dressed daily since the operation, and the incision, at the abdomen, has also been dressed daily, since the third day after the operation. The dressings are strips of adhesive plaster, a pledget of lint, and the uniting bandage. At the first dressing, the angles, and a considerable extent of the incision had united by the first intention.

Sunday, January 15th. On dressing the abscess to day, pressed out a large quantity of coagulated blood, mixed with pus, which seemed to relieve a little hacking cough, which has affected him for two or three days past. Pulse above ninety in a minute, much reduced in force, yesterday and to-day, but not much in frequency. For want of a time piece, denoting seconds, I was unable to ascertain the frequency of the pulse, with that particular accuracy which might have been wished. At ten o'clock in the evening he took twelve drops of laudanum, after which he slept better than any preceding night since the operation.

Monday, January 16th. On removing a strip of adhesive plaster, the aneurismal sack burst upon the top of the thigh, at the place where the shot entered. Grumous blood and pus, spouted at least four inches from the orifice; which, at first, excited some surprise, as it had a little the appearance of arterial blood. At least one pint of coagula was removed at this dressing, by my thumb and fingers, together with my knife and scissors, principally at the external orifice. Heat and pulse much reduced at this dressing. Ordered one table spoonful of the vinous tinct. of bark. Bowels somewhat constipated; nothing having passed for the last thirty hours. Ordered an enema of sulph. sodæ to be administered; pulse near one hundred in a minute.

Tuesday, January 17th. Abscess dressed twice; pulse very much reduced; patient much affected with cough. Ordered bark and wine to be given every hour, and an anodyne in the evening.

Wednesday, January 18th. Again repeated the thermometrical experiment:—

Temperature of the room at one o'clock		68°
do	under the tongue	100°
do	between the toes of the right foot	93°
do	do do left do	94°½
do	above the right knee	97°
do	do left do	98°

Bark and wine continued hourly, through the day. Cough increases, with a hectic flush in the afternoon. Bowels constipated. Ordered an enema of amylum in the evening. Pulse quick and fluttering ; an hundred in a minute. At ten o'clock gave an anodyne as usual.

Thursday, January 19*th.* Appearances much the same as yesterday. Had a voluntary stool in the evening ; medicines continued ; extremities cold, right foot painful ; a want of action in the toes ; directed the foot to be bathed with brandy and the tinc. opii.

Friday, January 20*th.* Cough somewhat abated ; rested well the preceding night ; tongue less coated, pulse good, about eighty in a minute. Complains of severe pain in the knee, calf of the leg, and foot, both at and after dressing. Heat much diminished ; the right foot indicating a disposition to mortification :—

Temperature under the tongue - -	99°½
do above the right knee - -	94°
do do left do - -	95°
do between the toes of the right foot	65°
do do do left do	84°

Medicines continued ; hot bricks applied to the feet, bathing and friction also continued.

Saturday, January 21*st.* Appearances of the wound, occasioned by the operation, and the abscess good ; discharges healthy pus from both, but much more from the latter than at any time previous. The foot and leg very painful and swelled, with a livid appearance, mortification having commenced in the little toe. Temperature of the foot much below the rest of the body. Ordered ten drops of laudanum to be given three times a day, and the bark and wine continued in increased quantities. The aneurismal limb bathed as before, and bandaged with a flannel roller. At ten o'clock in the evening, bathed the right foot and ankle with a saturated tincture of cantharides, which produced an efflorescence of the skin, but did not materially increase the heat.

Sunday, January 22*d.* Coldness of the foot and ankles continues. Mortification progresses in the toes. Partial sweats, particularly about the face, hands, and neck. Pulse strong and good, eighty in a minute. From the coldness of the weather, and the openness of the Hospital, it was very difficult to keep the room of an equable temperature :—

Temperature of the room - - -	49°
do at the right ankle - -	63°
do left do - - :	84°
do between the toes of the right foot	56°

At eight o'clock in the morning, ordered the right foot and ankle to be bathed with ol. teribinth. after which to be bathed hourly with a saturated tincture of cantharides.

Monday, January 23*d.* From the internal exhibition of medicine, and from the local applications that were made, the heat of my patient is very much increased :—

Temperature	of the room at one o'clock	49°
do	under the tongue	103°
do	between the toes of the right foot	64°
do	at the right ankle	64°

The variation of heat is dependent upon a variety of circumstances, particularly that of dressing the patient; the exposure which it occasions, uniformly produces a reduction of temperature. Pulse good; medicines continued. Though the action of the foot is somewhat increased, not being blistered by the tincture of cantharides, I applied a vesicating plaster over the whole foot and ankle. From fear that the pressure of the dressings upon the thigh might contribute to produce the coldness and swelling of the foot, the bandages were loosened last evening. To day the suppuration is very extensive, probably in consequence of loosening the dressings; nearly half a pint of bland healthy pus discharged. The os femoris is discovered to be bare for two or three inches, and the periosteum to be destroyed. Firm and tight dressings were again applied. The ligature upon the artery came away this day, and the wound, occasioned by the operation, appears well, having completely healed, except the little place occasioned by the ligature.

January 24*th*, 25*th*, *and* 26*th.* Colliquative sweats supervene. The situation of the patient now becomes quite precarious. Tonics and stimulants are continued in large quantities, with the addition of columb. and acid. sul. aromat. injections are administered according to circumstances. The blistering is continued upon the foot and leg, without arresting the progress of the mortification. By these means the action of the system is kept up. Considering the situation of the patient, his pulse continues remarkably good. Appetite, which has hitherto been very poor, is now a little better. Food principally milk porridge, occasionally a little chicken tea or beef soup.

Friday, January 27*th.* The wound at the abdomen is now quite well. Cough increases; pulse feeble, frequent and quick. A copious discharge of pus from the abscess, at least half a pint, which does not appear so well. It is thin, flaky, and smells disagreeably. The heat of the aneurismal limb, as far as the knee, somewhat increased, and about the same as the other. At ten o'clock in the evening:—

Temperature of the right knee - -	96°
do left do - - -	97°

Vesicating plasters and other medicines continued.

Saturday, January 28th. Pulse stronger, and much less frequent. Appetite somewhat increased. Expectoration considerable. Had two voluntary discharges from the bowels. The discharge from the abscess not so great; appearance of the pus much better, quite homogeneous, and perfectly bland. The blisters having now drawn tolerably well are removed, and the parts dressed with basilicon sprinkled with cantharides.

January 29th and 30th. Appearances much the same. Bowels which have all along been constipated, are now becoming rather loose. Appetite better; ate some beef steaks to day (Monday) with great relish. The gangrenous process still progressing. Medicines continued half hourly, in as large quantities as the stomach will bear, and as much wine and brandy as the patient will take.

There has, apparently, been a great want of action in the parietes of the aneurismal sack and parts adjacent, for a long time; perhaps owing to the pressure of firm coagula, so as to destroy the action and produce a sloughing of the neighbouring parts. With the coagula, some portions of mortified substance were discharged daily, which, from their near resemblance, it was difficult to distinguish. To obviate this, after syringing the absces with soap and water, and washing it with diluted brandy, I dressed it several times with cinch. off. in its pulverulent state, which, I think, acts both as a tonic and absorbent in abscesses and ulcers. How far it was beneficial in this case, is difficult to determine; in other cases, I have found the external application of it particularly serviceable.

Tuesday, 31st. The hole, at the point where the shot entered, has closed. The discharge of pus continues copious, say half a pint per diem. The foot and leg worse (especially the latter) which has become livid, quite to the knee, with a fluctuation about the capsular ligament. Medicines continued.

Wednesday, February 1st. Appearances of the patient much worse. Sphacelation progresses rapidly. Leg very much swollen, and discharges a bloody, watery fluid. The temperature of the foot and leg reduced to an equilibrium with the surrounding atmosphere. At one o'clock, P. M. applied a blister above the knee, and a fermenting cataplasm to the leg and foot, with the hope of arresting the progress of mortification; but all to no purpose. The abscess this evening discharges a thin, bloody, fœtid matter; the external opening much dilated, and the parts within discoloured. Pulse small and frequent, about one hundred and thirty in a minute. Appetite failing; medicines continued as before.

Thursday, February 2d. Mortification and swelling increase and extend quite to the abdomen. Scrotum discoloured and suffused with bloody lymph. Partial sweats continue. Pulse rapid and almost imperceptible. Countenance much altered; but cheerful. Very little appetite; ate some roasted apples to-day with relish. The yeast poultices are discontinued, and the limb sponged with a tincture of myrrh; afterwards covered with black oxyd of carbone and bandaged. Medicines, brandy, &c. continued, in as large quantities as the stomach will bear.

Friday, February 3d. Patient still lives; appearances much the same as yesterday; the whole abdomen much swollen. Loaths both food and medicine. Stimulants, however, are continued in large quantities, and brandy in particular every few minutes.

Saturday, February 4th. At five o'clock in the afternoon, under the treatment of yesterday, my unfortunate patient died, retaining the full exercise of his intellectual powers until the last.

On Sunday morning, in presence of Dr. Harris, I examined the body of the deceased, and am happy to state to the world, although the event of this case was unfortunate, that so far as relates to the operation all was well. It succeeded most perfectly.

Appearances on Dissection.

All the parts in the neighbourhood of the artery were uninjured by the operation, except those necessary to be divided. The inflammation in the cavity of the wound, produced a slight adhesion of the omentum to the peritoneum, say from one third to half an inch. Except this morbid adhesion (which was not of the least consequence) all the parts were nearly in their natural state. The internal iliac artery was not so much enlarged as might have been expected. From the putrid state of the limb, the texture of which was very much destroyed, I was unable to trace the minute ramifications of the internal iliac artery into the substance of the thigh, but presume, from the size of the trunk, which was very little larger than its fellow, that they were not much dilated. I have now before me a preparation of the artery and vein, with the trunk of the principal branches of the artery, from the bifurcation of the aorta, to the popliteal artery of the ham, with the external and internal iliac of both sides.

The disease was evidently a false aneurism, an aneurism of the diffused kind. The coats of the artery were not in the least dilated; the bleeding point of the artery was four and an half inches from the bifurcation of the arteria iliaca communis. The mouths of the divided artery were an inch and an half asunder, gaping, with their edges ragged. The ligature was made one inch below where the internal iliac is given off, which of course divided the artery; the mouths of which, at this place, were separated half an inch, standing open, their edges smooth. The accompanying vein and nerve were sound. The thigh very much reduced in size, being diminished full three fourths since the operation, by the general wasting of the system, the absorption of the parietes of the sack, and the sloughing of the muscles. The periosteum destroyed for a considerable extent and the os femoris carious.

Queries. What was the cause of this aneurism? Did it result from a direct wound of the artery, by the passage of the buckshot? By an erosion of the artery subsequent to that period? Or by some cause independent of either?

Did the mortification depend upon the obstruction of the circulation, occasioned by the operation? Or by the destruction of the minute ramifications of the internal iliac artery, occasioned by the pressure of the aneurismal tumour upon the surrounding parts, and by the subsequent sloughing?

Might not this patient have been saved by an earlier operation?

Sacket's-Harbour, February 22, 1815.

READINGS AND OBSERVATIONS.—No. IV.

SYPHILIS.

It has been questioned by many whether it be possible to affect the system of a nursing child by quicksilver, through the medium of the mother. The following case proves the affirmative.

In the year 1808 a woman, who had an infant about six weeks or two months old at the breast, received the venereal disease from her husband. Being ignorant of the nature of her complaint, and trusting to common applications for a cure, it was allowed to proceed for some months before she applied to the writer of this article. The disease was found by him to exist locally to a great extent, accompanied with ulcers at the corners of the mouth, and on the nipples and breasts. The matter of these ulcers coming into contact with the child had produced the same disease in and about its mouth, neck, and breast to a great degree. In fact, the parent and offspring together presented a spectacle equally pitiable and disgusting. The child was ordered to be kept at the breast, and the mother was treated in the usual way by frictions, pills of sub-muriate of quicksilver, &c. A moderate salivation ensued, during which the face of the infant became swelled; a profuse discharge of saliva followed; the sores gradually healed, and they were both cured at the same time.

PARALYSIS.

The use of splints in cases of paralysis of the muscles of the extremities, as is sometimes seen in painters, has been recommended by some English author, whose name is not now recollected. The writer of this article can bear testimony to their utility in a case of this kind which a few years since fell under his care. The obvious advantage resulting from their application induced him to try them in the following case.

A young woman placed her arm on the back of a chair, laid her head upon it and slept in that position for an hour or more. On awaking, the fore arm felt benumbed, and she was unable to move the hand. When the arm was raised the hand would fall backward or forward according to the position; both the flexor and extensor muscles having entirely lost all voluntary motion. She was ordered friction with flannel, which not producing any amendment, was followed by the use of the common stimulating embrocations, applied of her own accord. The arm was thrice blistered, and repeatedly rubbed with alcohol, ammoniated oil, camphorated oil, and diluted tincture of cantharides, accompanied with affusions of cold water twice a day, for nearly a month, without any obvious benefit. It was then bound with long splints, as is practised in the case of a broken limb, the apparatus being removed for a short time every fourth or fifth day. In the course of ten days she could support a weight of three or four ounces between the thumb and finger, and move the hand. From this time she rapidly recovered the use of it, and two months after the muscles had regained their usual power. The rapid amendment which followed the use of the splints, and which was such as to excite the surprise of the patient herself, would lead us to believe that they had some considerable influence in accelerating the cure. It should have been remarked, that they were sufficiently long to reach from the elbow to the ends of the fingers.

DIGITALIS PURPUREA.

Dr. Withering has generally among us had the credit of introducing this article into the materia medica; but it appears to have been known and used in medicine long before his time, for its properties are mentioned by Salmon, whose work appeared before that of Bates. Hence it must have been prescribed in the same diseases that it is at present more than a century ago, though perhaps in a more homely style. His formula is as follows: "Take of fresh foxglove twelve ounces, of liquorice four ounces, of anniseeds two ounces, of strong beer twelve pounds, digest for nineteen days and distil, s. a." The distillation was not the best part of the formula. The strong and quaint terms which he uses in describing its effects, are amusing. "It takes away wheezings and shortness of breath, and is prevalent against most distempers of the lungs, which obstruct or hinder breathing. A dose of from one to two ounces with ten or twelve drops of sal armoniack *(liquid carbonate of ammonia,)* thus taken is good against quinsies, pleurisies, and the like. If you give the syrup of foxglove with this, it most effectually cures consumptions. I have done such great cures with it thus given in consumptive people, and such as have been given over as incurable, as have put me into an amazement. But then it is to be given for some time till it is manifest that the obstructions causing the consumption are perfectly removed and the patient evidently regains flesh and strength." Z.

DESCRIPTION OF A SUBSTITUTE FOR LEECHES.

BY JOHN WELSH, *Surgeon*, *Haddington*.

[Extracted from the Edinburgh Medical and Surgical Journal.]

In a late pamphlet on cupping, by Mr. Mapleson of London, it is affirmed, that the adroit performance of the operation continues to be confined to a few individuals in that capital. I never witnessed the operation by any of those gentlemen, but it has been minutely described to me by a person, who had undergone the operation himself and seen it repeatedly practised upon others. The neatness and expedition with which they abstract even large quantities of blood, fully justify the frequent recourse which is had to their assistance in London practice ; and notwithstanding the great number of scarifications, inflicted by three or four applications of a scarificator, containing sixteen lancets, the pain, I am assured, is so inconsiderable, that patients readily submit to the operation. Mr. Mapleson's professed object is to instruct surgeons in the most approved method of cupping ; but it may be doubted, whether any directions, however explicit, or even the occasional performance of the operation with the apparatus now in use, can enable surgeons in general to rival, in any degree, the dexterity of those who practise this operation exclusively.

I have long been in the practice of procuring a discharge of blood both from scarifications and the bites of leeches, by means of cupping glasses, originally intended to be used with an air-syringe, employing merely suction with the mouth ; and I can with much confidence recommend this, probably the most ancient mode of cupping, as the simplest, the most certainly effectual in the hands of those not much accustomed to the operation, not liable to the obstructions, which apply to the air-syringe, and capable of wholly superseding the spirit of wine-lamp, in the use of which alone the dexterity of the professional cupper is admitted to consist.

As the brass cap and screw with which the cupping glass used with the air-syringe is mounted, renders it rather inconvenient for suction by the mouth, I have lately had some formed entirely of glass, of the ordinary sizes and shapes, except that they have a cylindrical mouth-piece, about an inch in length, projecting from their apex. On the superior aperture of the mouth-piece, a valve is formed, and at any time very easily renewed, by tying on a small bit of smooth bladder. It might perhaps add to the neatness of the operation, to have the tubular part of the glass of a much greater length, by enabling the operator to keep his head further removed from the patient. But this, in some situations, might prove inconvenient and would render the glasses less portable and more liable to accident.

These cupping glasses may be had at Messrs. Boogs, cutlers, Edinburgh, with whom one is left as a pattern; and any gentleman, without proceeding to the operation of cupping, may satisfy himself of their simplicity and effect, by merely dipping the edge of the glass in water, applying it to the palm of the hand, and employing a little suction; or by sucking the air from it, inverted upon water, when it will be found, that it may be entirely exhausted by two moderate inspirations; and that the valve completely excludes the air upon the suction being discontinued. This facility of exhaustion suggests the necessity of caution, not to carry it too far in cupping, which, as Mr. Mapleson observes, impedes the flow of blood, by compressing the cutaneous vessels.

The scarcity of leeches for some years past, appears to me to impose it as a duty upon surgeons to become more familiar with the practice of cupping, more especially among the lower orders, who must otherwise, from not being able to obtain leeches, often forego the advantages to be derived from local blood-letting. When, from timidity of patients, or other reason, leeches are prefered, instead of the tedious and often ineffectual fomentation of the bites, a sufficient quantity of blood may, in general, be obtained in a very short time with these glasses, particularly if the leeches have been applied in groups, so that each glass may include four or five bites.

Official Report to the French Institute, by Messrs. Vauquelin, Pinel, and Percy, on the manuscript work presented to the Institute, by M. Orfila, entitled, "Toxicologie Générale."

[From the London Medical and Physical Journal.]

The work which has been entrusted to us to revise, is comprised in two volumes, each divided into two parts; the first comprehending the mineral poisons, and the second, the vegetable and animal poisons.

A complete treatise on this interesting subject, was a desideratum to science and medical jurisprudence; all those which we previously possessed, were either incomplete or inaccurate; it was in vain that we looked into some of them for the best method of ascertaining the nature of poisons; in others, there was no description of the organic lesions produced by poisonous substances, while the collection of every peculiar effect exhibited, was far from affording adequate materials with which to meet every emergency.

It was necessary, therefore, in order to produce a book on the subject, such as might have been expected from the present state of science, to undertake a series of multiplied and very delicate experiments; this is precisely what M. Orfila has had the courage to undertake, and he purposes to attain the highest possible degree of perfection.

He first describes, with care, the physical and sensible characters of poisons in their natural state; he afterwards makes known the chemical properties of these substances, noting very exactly the phenomena which they present on the application of the greatest possible number of reagents. But what renders this part of his work the more interesting is, that he details the differences which any given poison, when mixed with alimentary substances of various kinds, exhibits with the same reagents.

Poisons thus mixing themselves in the stomach and intestines with various liquids, disguise their properties, either by combining with or decomposing them. The antecedent researches of the author would have been insufficient for his purpose, if he had not studied, in a particular manner, the modifications which the bile, the saliva, the gastric juice, &c. are capable of imposing upon poisons. In making these experiments, M. Orfila varied the quantities of the poison from a dose so small as to be incapable of producing death, up to a quantity more than sufficient; circumstances which frequently produce differences in the effects occasioned by the reagents.

When the author has well ascertained the characters of the poison in its natural state, and has pursued them through its mixtures or combinations with the alimentary substances, as well as with the humours which are met with in the stomach and intestines, he endeavours to ascertain the manner in which it acts in the animal economy and the effects which flow from it. This is the physiological part. Sometimes an inspection of the organs affected, has justified his prediction; but frequently also, the mode of action in most poisons is unknown to us; fortunately this is not the most imperfect branch of toxicology.

That which concerns us all most closely, is the art of preventing, arresting, or remedying the deleterious effects of poisons; thus M. Orfila has collected, with great care, whatever has been written hitherto on counter-poisons; subjected to new trials, approved antidotes, and frequently in discovering their fallacy, he has undertaken new inquiries, which have sometimes been followed by the happiest results. It was thus that he ascertained that every thing which has been suggested as the counter poison to corrosive sublimate was really useless, and that the true antidote to this dreadful poison was albumen dissolved in water. He made this experiment not only on animals, but he had an opportunity of verifying it on the human subject. It was in this way also that he ascertained by repeated experiments, that the antidote to verdigris is sugar, a happy discovery; and which certainly could not have been attained by theory.

In the fourth article upon each poison, he traces, in some measure, for the use of police officers, the steps which they ought to take to ascertain the nature of the poisonous substance; whether it has been found in its natural state in any person's house, or has been mixed with foreign bodies; whether it has been vomited, or, finally, found in the stomach or other organs of an unfortunate person who has died from its effects.

We cannot too highly praise the minute order and precision which the author has observed; and he seems to have felt deeply the serious consequences of an erroneous judgment in such cases.

The first volume of M. Orfila's toxicology, contains four classes of mineral poisons, viz. the mercurial, the arsenical, the antimonial, and the cuprous. The second volume treats of mineral poisons also; and the third, of vegetable and animal poisons. We have no hesitation in saying, that the work is as perfect as the present state of our chemical knowledge will admit; and we can assert, that the individual researches of the author have corrected many old errors, and added a great number of truths highly valuable to medical men, who are frequently called to give their assistance to persons poisoned; to the officers of police, whose business it is to see if there has been really poison employed; and, finally, to humanity itself. But we ought not to expect, that the part which treats of vegetable and animal poisons should be equally complete, because the nature of these poisons not being so well known as that of the mineral, their way of acting on the organs, consequently, is more obscure, and the methods of remedying their effects much more difficult. It is doing much, however, to have opened the career, to have pointed out the progress and indicated the means of attaining the desired object. Time and experience will, it is hoped, gradually perfect this important branch of toxicology.

The second part of the first volume contains a description of the action produced on the animal economy, by the preparations of tin, zinc, silver, gold, bismuth, the concentrated mineral acids, the caustic alkalies, phosphorus, cantharides, lead, iodine, and an appendix on the counter-poisons of corrosive sublimate, arsenic, and liver of sulphur.

He begins with the chemical and medico-legal part, and afterwards examines the effects of the poisons in a physiological point of view. This last part, being almost entirely new, we shall present a succinct extract to the class.

1. The muriate of tin injected into the veins, to the quantity of three fourths of a grain, acts promptly on the nervous system, and produces death in ten or twelve hours. Introduced into the stomach, it destroys life by inflaming and corroding it. Six experiments gave the same results.

2. A concentrated solution of sulphate of zinc acts by stupefying the brain, when it is injected into the veins; when introduced into the stomach to the amount of an ounce, it produces

vomiting only; but if the œsophagus be tied, the animal dies in two or three days, and the stomach is much inflamed. Six experiments also confirm these facts.

3. One third of a grain of nitrate of silver, dissolved in two drachms of water, introduced into the circulation, produced death in five or six hours, by acting on the lungs and nervous system. When thirty-six grains were introduced into the stomach, it was not absorbed, and the animal did not die until the fourth or fifth day, in consequence of the inflammation produced by this caustic. Six experiments gave the same result.

4. Three fourths of a grain of muriate of gold, dissolved in a drachm of water, and injected into the veins, gave death in six or seven hours by attacking the lungs forcibly. When introduced into the stomach to the amount of twelve grains, the animal died in five or six days, and the stomach was corroded; consequently there had been no absorption. Five experiments supported these facts.

5. The nitrate of bismuth, injected into the veins, has its chief action in the nervous system and kills animals. When introduced into the stomach it inflames and corrodes it, acting, at the same time, on the lungs and destroying life very speedily.

6. Some drops of an acid or alkali, injected into the veins, produce death instantly by coagulating the blood; the sulphuric acid carbonizes it, as it does in common vessels. Introduced into the stomach it corrodes and perforates it, and the animals, so treated, die amidst bloody vomitings, frequently attended by the most frightful convulsions.

The coagulation of the blood by alkalies is remarkable, since they prevent this fluid from coagulating, when it is drawn from the human body. It results from all these facts, that the same poisonous substance can exert its deadly influence on this or that organ, according to the point with which it has been brought in contact.

7. Ammonia and its sub-carbonate, injected into the veins, also coagulate the blood; but they act strangely on the nervous system. When introduced into the stomach, in a dose of a drachm or two, they produce death in a short time, and act upon the brain.

8. The muriate of barytes, injected into the veins, introduced into the stomach, or applied externally, kills animals in a very

short time, in the midst of frightful convulsions, acting on the nervous system. Six experiments proved this property. Mr. Brodie had already announced a part of these results.

9. Phosphorus, dissolved in oil and injected into the veins, produces instant death, by being converted into phosphoric acid, which is exhaled through the nostrils, as Mr. Magendie had already witnessed.

When introduced into the stomach in small cylinders, it passes into the state of phosphoric acid, which corrodes the texture of this organ, and occasions death in a day or two; the stomach of the animal is found to contain less phosphorus than was employed.

When the phosphorus is dissolved in oil, before it is given to the animal, it is transformed into phosphoric acid. Life is destroyed in a few hours, and the stomach is found full of holes. Six experiments proved the fact.

10. The acetate of lead, when introduced into the stomach, to the extent of an ounce and an half, occasions, abundant vomitings and death intervenes ten, twelve, or fifteen hours afterwards. On opening the body, a true inflammation of the parts which compose the digestive canal, is found.

If it be curious to endeavour to ascertain the effects produced in the animal body by poisonous substances, when introduced either by the blood vessels or by the mouth, it is still more curious and certainly more useful to find out the means of preventing the deleterious effects of these bodies, or, at least, of arresting their progress when they have already commenced; this has been inquired into, in the medical department of M. Orfila's work.

1. Milk is the true counter-poison of muriate of tin, a substance which frequently falls in the way of individuals. The milk is completely coagulated by this salt; the coagulum contains abundance of oxide of tin and muriatic acid, and this coagulum is not poisonous. Three experiments confirm this fact.

2. The muriate of soda is the true counter-poison of the nitrate of silver, since it prevented the corrosive effect of this salt. Two experiments demonstrated this.

3. Calcined magnesia proposed by Pelletier as the most certain method of arresting the action of the acids, succeeds, in

fact, very well; several experiments have demonstrated it, but this remedy must be administered very speedily.

4. The sulphates of soda and magnesia are the true counter-poisons of the salts of lead and barytes. There result from the reciprocal action of these substances purging salts, which produce the evacuation of abundance of sulphates of barytes and lead. These antidotes must be employed in great quantities, and at frequent intervals.

M. Orfila discovered that the sulphuret of potash recommended by Navier to check the effects of the metallic salts, was of no use.

5. The acetic acid is the most efficacious remedy in cases of poisoning by the alkalies. M. Orfila made several experiments to prove this.

6. Iodine, having produced on dead, organic substances, effects very analogous to those exercised by the oxy-muriatic acid, M. Orfila was curious to know what were its effects in the living animal economy. When introduced into the stomach in a small quantity, it acts like a slight stimulant and produces vomiting. In the dose of a drachm, it constantly kills animals, whose œsophagus has been tied, producing ulcerations in the mucous membrane. In the dose of two or three drachms, it acts in the same way on animals, whose œsophagus has not been tied, and which are several hours without vomiting. It rarely produces death when it has been administered in the dose of a grain or two, and when the animal rejects it in a short time by vomiting. It never destroys life when applied externally. It acts upon man as upon dogs. M. Orfila having once taken two grains of iode, experienced nausea; on another occasion, having taken four grains, he had nausea, with symptoms of choaking, vomiting, and a slight oppression; when he took six grains, he had the same symptoms, besides an acceleration of the pulse and symptoms of colic.

In an appendix to this work M. Orfila shews, that charcoal is not the counter-poison of corrosive sublimate and arsenious acid (white arsenic) as M. Bertrand had announced. 1st. Because animals that took six grains of one or other of these poisons, mixed with four times as much charcoal as M. Bertrand employed, died in a day or two, when the œsophagus was tied, and the stomach was strongly corroded. Now what constitutes the essence of a counter-poison of corrosive sublimate, is the prevention of corrosion. 2d. Almost all the animals which took this mixture, and whose œsophagus was not tied, died, after having vomited several times; the stomach was found greatly inflamed.

Two only, out of twenty of the animals which were subjected to these experiments, escaped, and they instantly vomited the poison enveloped in the charcoal.

In order to prove that the charcoal had acted as an envelope only, six grains of the same poison were given to those animals, enveloped in clay, which they vomited instantly and were restored. The water of charcoal is not more efficacious.

In the same appendix M. Orfila established, by experiment, 1st. that the sulphuret of potash is an energetick corrosive poison; 2d. that, in the dose of half a drachm, it produces death in eighteen or twenty hours, when the œsophagus is tied, by determining inflammation, or ulceration of the membranes to the stomach, and acting on the nervous system; 3d. that, in the dose of three or four drachms, it kills animals in three or four hours, if they are prevented from vomiting. The author made all his experiments on dogs.

The researches, of which M. Orfila has made up the second part of his work, being of so frequent application, and so immediately useful to the preservation of human life and to medical jurisprudence; the simple and methodical manner, in which the author has treated this interesting subject, together with the pain and disgust attendant upon these kinds of experiments, will, doubtless, determine the Institute to grant him permission to publish his toxicology with their special approbation; and his zeal, will, consequently, be redoubled in the perfection of that department of his experiments, which shall include the vegetable and animal poisons.

(Signed) PINEL, VAUQUELIN, PERCY,

The Institute approves of the above report and adopts its conclusions.

G. CUVIER, *Secretary*.

OBSERVATIONS ON THE FUNCTIONS OF THE BRAIN.

BY SIR EVERARD HOME.

[From the Philosophical Transactions of the Royal Society of London, for the year 1814.—PART II.]

I. *Effects Produced by an Undue Pressure of Water upon the Brain.*

BEFORE I enter into the particular effects that take place when pressure is made upon the brain by means of water, it is necessary to mention that sudden pressure of any kind upon the cerebrum, takes away all sensibility, whether made upon the external surface through the medium of the dura mater, or upon the internal parts through the medium of the ventricles, and sensibility returns as soon as the unusual pressure is removed.

Faintness is the consequence of the pressure to which the cerebrum has been accustomed, being suddenly taken off.

I am induced to believe that pressure to a certain degree uniformly kept up, is necessary for the performance of the healthy functions of the cerebrum; and any increase or diminution of this pressure puts a stop to them. It is asserted, that in addition to this pressure, the pulsatory motion of the blood in the arteries of the cerebrum is also necessary; but the late John Hunter, whose accuracy in a point of this kind is not to be doubted, retained his senses, although the heart had apparently ceased to act.

Although insensibility is the common effect of undue pressure upon the cerebrum, it appears, from what will be stated, that it is not a necessary consequence of undue pressure upon the cerebellum.

The facts which have been stated, appear to point out the use of the water in the third ventricles of the brain, and they account for the great variety which is met with in the form and extent of the posterior cornua of the lateral ventricles, their size varying according to the quantity of water which is necessary to keep up the pressure required.

The size of the ventricles would appear to be very immaterial, since even when they are increased so as to contain above six ale-pints, the functions of the brain are all carried on, and the growth of the body proceeds; but, after the skull is completely ossified, an increase of two or three ounces produces insensibility.

That the ventricles should admit of being increased to so great an extent, without any of the senses or faculties of the brain being destroyed, is in itself a curious fact, and of so much importance with respect to the physiology of the brain, that I shall detail the two following cases, which illustrate one another.

In the one, the accumulation of water proceeded, as it will appear, as far as it could go without materially impairing the organ, it there stopped, and the boy grew up with all his faculties. In the other, the water continued to increase; the substance of the cerebrum was absorbed, and the faculties of the brain were destroyed.

A boy, at a month old, had so rapid an increase of the size of his head, as to evince an accumulation of water in the brain; and when he was five years old, the head was so large that the parents, judging from recollection, believe that it never after increased, It was so transparent, that when exposed to the sun, the rays passed through it as they would through a horn-lantern. He was unable to walk. At this age, he caught the natural small-pox, which was so violent as nearly to prove fatal. Upon his recovery the head shewed no disposition to increase, and the child, in all respects, began to improve, and, for the first time, learned to walk. At fourteen the skull appeared completely ossified. At nineteen years, the time I saw him, he was five feet six inches high; his head measured in circumference 33½ inches. He had grown in the course of the last year about two inches, which is more than he had usually done in any one year.

All the organs of sense are entire; savory food is agreeable to his taste, but he is moderate in eating. His sight is good, but looking with attention at objects more than half an hour appears to strain his eyes. His head is so heavy, that the muscles of the neck are unable to support it for many hours together; when he lies down the head is supported by another person.

He sleeps with most ease on the right side, and the left side of the head appears to the eye to be rather the largest. In lying down, there is what he describes to be a momentary thrilling heat felt on the upper part of the brain, in the line of the longitudinal sinus. Lying on his back strains his eyes so much, that he cannot continue in that posture ; stooping forwards brings an oppression on the eyes. The least weight in his hand, as a teacup, makes it tremble ; all sudden noises jar his head, and produce giddiness. When he falls down, the jar renders him insensible ; at one time this was the case for fifteen minutes, without being attended with any bad consequences. His head aches when exposed to heat. He has had no illness since the small-pox. His sleep is easily broken ; he never dreams. He is fond of reading and writing ; has a taste for poetry, and can repeat verses from Cowper. His memory of common things is very good. He never expressed any attachment or passion for women. He is of a mild disposition, but, when irritated, his whole frame is in a state of agitation, which, however, soon goes off.

In another boy, the enlargement of the head was perceived at three months, and increased for three years, and then appeared to be stationary ; and the child, till that period, was sensible. The upper part of the skull from that time began to ossify, and in three years more there was only an irregular space at the fontanelle, and a small space between the two portions of the os frontis remaining open. The child continued sensible till three years old, and then became gradually less so ; did not know what it did ; heard sounds, but could not see. At six years old he died.

The child was three feet three inches high, the skull twenty-seven inches round ; the water contained in the two lateral and third ventricles, was six ale-pints and an half in quantity. The cerebrum formed a thin case of medullary substance surrounding this cavity. The cerebellum was entire.

The weight of the whole brain was two pounds, three ounces, and one drachm.

The weight of the whole brain of a child between six and seven years, two pounds and twelve ounces.

The preceding facts explain satisfactorily that the cerebrum is made up of thin convolutions of medullary and cortical substance surrounding the two lateral ventricles, which are unfolded when the cavities of those ventricles are enlarged ; and in this unfolded state, the functions belonging to this part of the organ can be carried on.

Although the quantity of water may be so much increased without material injury to the functions of the brain, when the skull is not ossified, yet, after that period, even a few ounces in the lateral ventricles have been known to produce so much undue pressure, as to bring on head-ach, general uneasiness, a sensation as if the head were too large, loss of spirits, convulsions, loss of memory of recent events, idiotism, insensibility and death.*

When the water, instead of being contained in the general cavities of the lateral ventricles, is principally confined to their posterior and anterior cornua, the effects sometimes are occasional constipation, pain in the bowels and lower part of the belly.† When accumulated in the third ventricle, without any increase in the lateral ones, distressing pain in the head, loss of speech, and insensibility have occurred.‡ When accumulated in the ventricles of the brain, and also under the tuberculum annulare, painful sensations in the stomach, bowels, lower belly, and across the legs, have been met with.§ When not only in the ventricles, but between the tunica arachnoides and the pia mater over the hemispheres, and also upon the tubercula quadrigemina, the apparent consequences in one case, were depression of spirits, pain in the back of the head and mania.* When in the ventricles, and also between the tunica arachnoides and pia mater, and between the dura and pia mater, melancholy, imbecility, apoplexy, and paralysis of one side, have been the accompanying symptoms.† When in the ventricles, where there has been an unusual vascularity of the dura mater, violent affections of the præcordia have occurred in the night, during sleep, which have led to suicide.‡ When between the dura and pia mater in considerable quantity, a state of melancholy and imbecility of mind has been met with.§

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* One ounce and an half of water in the lateral ventricles at four and an half years old, were accompanied by pain in the head and great general irritability; five ounces at seven years, by insensibility for fourteen days before death; two ounces and an half, at nine years, by violent head-achs and unwillingness to be moved, and stupor; three ounces, at eleven years, were attended with loss of recollection and recent events. This began at the age of ten. A stupor of twelve days preceded death. Four ounces, at eleven years, were followed by the sensation of the head being too big, loss of speech, insensibility, convulsions Eight ounces, at fourteen years, were accompanied by a state of idiotism for ten days and then death.

† Five ounces of water, contained principally in four small cells, two in the anterior, and two in the posterior cornua of the lateral ventricles, at six years of age, were the only evident cause of pain in the lower part of the belly, occasional constipation, and violent pain in the bowels.

‡ Two ounces of water in the third ventricle, enlarged by the layers of the septum lucidum being separated, at thirty years of age appeared to produce distressing pain in the head loss of speech and insensibility. Two drachms of water in the third ventricle of an aged dog were attended for four years by fits like apoplexy, pain in the head only relieved by opium, convulsions and death.

§ Two ounces of water in the lateral ventricles, one and an half ounce under the tuberculum annulare, between the tunica arachnoides and pia mater, at five years of age, were followed by painful sensations in the lower belly, stomach and bowels. Two ounces of water in the ventricles,

II. *Effects produced by Concussion of the Brain.*

Concussion of the brain produces delirium and coma: these symptoms go off; they sometimes in a few days return and prove

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one under the tuberculum annulare, at three years, appeared to produce not only painful sensations in the lower belly, stomach, and bowels, but pain in the head and pain across the legs, as if they were cut with an instrument.

* Water, in the third ventricle, in sufficient quantity to separate and keep apart the thalami nervorum opticorum, also between the tunica arachnoides and pia mater, over the hemispheres and under the tubercula quadrigemina, in an adult, were attended with pain in the back of the head, low spirits, and, after drinking wine, very high spirits. This ended in mania, and after three months in death.

† Two ounces in the lateral ventricles, one between the dura and pia mater; the space between the tunica arachnoides, and pia mater loaded with it, the consequences of attacks of inflammation upon the coverings of the brain in an adult, were attended by these symptoms. The paralysis took place after the last apoplectic fit, and death ensued.

‡ Two ounces in the lateral ventricles, and a very unusually vascular state of the dura mater in an adult, was followed by these effects.

§ A gentleman fell from a horse. From that time he had head-achs, gradually became melancholy and imbecile. He lived three years in that state. Four ounces of water were found between the dura and pia mater, on the right side. There was an exostosis one eighth of an inch long from the parietal bone with a sharp point, in contact with that part of the dura mater under which the water lay. There were four ounces in the lateral ventricles, the posterior cornua were very small; there were also three ounces on the basis of the skull.

fatal.* In the torpid state, commonly attendant upon any violent shake being given to the brain, the senses are so much impaired, that little information can be gained respecting the effects produced on the internal organs. The bowels have been found, under such circumstances, to be acted upon by aperient medicines with great difficulty.†

III. *Effects produced when the Blood Vessels of the Brain are Preternaturally Dilated or Diseased.*

Sudden dilatation of the blood vessels of the cerebrum, in consequence of exposure to the sun, is sometimes accompanied by delirium, loss of speech and the power of swallowing.

A dilated state of the veins of the cerebrum, has been attended with headachs, which are very severe when the body is placed in a horizontal posture.

When the smaller arteries of the cerebrum are preternaturally enlarged, while those of the cerebellum are not, delirium has taken place, followed by a fit resembling apoplexy, and a paralytic affection of one side.

An obstruction to the passage of the blood, through the right internal carotid artery, was attended by a succession of slight apoplectic fits, unaccompanied by any paralytic affection.

An aneurismal enlargement of both the internal carotid arteries to the size of marbles projecting into the cavernous sinuses, was the only apparent cause of attack of mania with consciousness of being insane.

IV. *Effects produced by Extravasated Blood.*

Blood in the lateral and third ventricles was attended by repeated fits of vomiting and coma. In the fourth ventricle, a fit, which in twenty-four hours terminated in death; under the anterior lobes of the brain by hiccoughs and stupor; under the cerebellum, by convulsions of the neck and body, with drawing up of the feet without stupor; in the folds of the pia mater covering one hemisphere, by a paralytic affection of the opposite side, without any other symptom.

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* This happened in two cases, and no appearances of alteration of structure in the brain were met with after death.

† A gentleman fell from his horse and had a concussion of the brain; while in this state, it required sixty grains of Jalap and twenty grains of calomel, to procure one evacuation from the bowels.

Blood in the folds of the pia mater over the posterior lobes of the brain, and serum in the cornua of both the lateral ventricles, were attended by giddiness, paralysis, straight objects appearing crooked, loss of memory, and at last idiotism. In the right thalamus nervi optici, extending into the lateral ventricles, by paralysis of the left side of the body, both eye-lids closed, the mouth drawn on one side, a perception of light with the right eye, but not with the left, succeeded by coma. Between the dura mater and skull covering the right hemisphere, by stupor which went off on its removal; but taking of the pressure produced faintness for a few minutes.

Coagulable lymph spread over the union of the optic nerves, the pineal gland, and tuberculum annulare, was followed by permanent contraction of the muscles between the occiput and vertebræ of the neck, dilatation of the pupil, and a great degree of deafness. Serum under the cerebellum by restlessness, convulsion, incessant talking, at times incoherent, and the eyes became insensible to light.

V. *Effects produced from depression, and by thickening of different portions of the skull.*

Unusual pressure of the skull upon the middle lobe of the brain, was attended with pain in the stomach, torpor of the bowels, nausea, retching, pain between the shoulders, and in the feet. On the upper part of the hemisphere, want of sleep, head-ach and stupor. Those effects went off upon its removal. On both of the anterior lobes of the brain, heaviness, loss of memory, depression of spirits, bordering on idiotism. On the anterior lobes of the brain, accompanied with water between the tunica arachnoides, and pia mater covering the superior part of the hemispheres, an apoplectic fit, heaviness, loss of memory, and a second apoplectic fit, which terminated in death. On the lower and lateral part of the left posterior lobe of the brain, uneasiness in the skin of the left cheek, extending along the chin, throat and trachea, hissing noise in the ears, inability to speak the words the person wished to articulate, using others in their place, although conscious of doing so, and unable to correct it. Numbness in the arms and legs. These effects ceased on taking off the pressure.

On the anterior lobes of the brain, both anterioly and laterally, with thickening of the pia-mater, spasms in the lower extremities, and total loss of memory, so that the person did not know what he had done a few hours before, although in other respects in health. On the lower and lateral portions of the anterior and middle lobes of the brain, head-achs, general wasting, irregularity in the action of the bowels; the feel of inability to swallow, and great distress in the act of swallowing, with great general irritability.

VI. *Effect of pressure from Tumours.*

An hydatid imbedded in the substance of the right hemisphere of the brain, was attended with violent head-achs, and occasional fits similar to those of apoplexy. A tumour in the substance of the posterior lobe of the brain, was attended with derangement of the functions of the stomach and bowels, double vision, and afterwards loss of sight. A tumour pressing on the left hemisphere, settled melancholy, drowsiness after dinner, requiring being carried into the air, which took it off, but it returned on coming back to the table. A tumour in the fourth ventricle, epileptic fits, soreness in the throat, and great pain in the act of deglutition. A tumour in the tuberculum annulare and water in the ventricles, pain in the head, stumbling in walking, the mouth drawn on one side, loss of sight of one eye, although the pupils were not affected; dulness in hearing, difficulty in swallowing, so as to die starved, with all the mental faculties entire.

VII. *Effects of injury to the substance of the brain.*

A deep wound into the right anterior lobe of the brain, attended with inflammation and suppuration, produced no sensation whatever; the senses remained entire, and the person did not know that the head was injured.*

The brain shooting out in the form of fungus, after the dura mater is wounded, has no effect upon any of the nerves, nor is it attended with sensation;* but the inflamed pia mater gives great pain.

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* From an explosion of gun-powder, a piece of copper three inches long was forced through the eye into the brain; but the arm being also wounded, the person never knew the eye to be injured. When suppuration came on he became insensible, and in a few hours died.

Loss of a portion of the medullary substance of the anterior lobe of the cerebrum produced no symptoms.† Loss of a portion of one of the hemispheres was attended with difficulty of swallowing for twenty-four hours, and slight delirium of short duration.‡ Ulceration of the anterior lobe of the brain as low as the anterior cornua of the lateral ventricle, but not communicating with it, paralysis of both arms.§

In a case of a penetrating wound into the right hemisphere of the brain with bone forced into its substance, while there was an opening for the discharge of matter, no effects were produced, except where the circulation was much increased, and then only head-ach, and numbness in the left side.||

VIII. *Effects of alteration in the structure of the brain.*

In a case in which the tuberculum annulare had undergone a change in its texture, and become so hard as with difficulty to be cut with a knife, a considerable quantity of earthy particles being intermixed with the medullary substance of the crura, and other parts of the cerebellum, and the cerebrum and upper part of the cerebellum unusually soft; the effects were, the boy had been an idiot from his birth, never walked, spoke, or understood what was said. Went often three days without food. At sixteen, when he

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* A fungus from the brain shot out through the dura mater after the operation of the trepan, upon the left parietal bone. The pia mater surrounding it was so sensible, that when touched, the pain was excruciating.

† A boy five years of age, fractured his skull, and wounded the brain; half an ounce came away. When he grew up he was more acute, and had a better memory than his brother.

‡ A fracture of the parietal bone, and wound of the dura mater produced the injury to the brain.

§ The ulceration took place in consequence of the trepan wounding the dura and pia mater.

|| A musket ball fractured and depressed a portion of the superior and posterior part of the parietal bone, but did not penetrate; an abscess formed in the substance of the brain, in which the broken portions of bone were lodged. In this state the person was capable of doing his duty as a naval officer from China to England, but died in consequence of an attempt to extract the depressed bone.

died, was no bigger than a child three years old, except the head, which was as large as it is usually at twelve.*

IX. *Effects of injury to the medulla spinalis.*

Pressure upon the medulla spinalis in the neck by coagulated blood produced paralytic affections of the arms and legs, all the functions of the internal organs were carried on for thirty-five days, but the urine and stools passed involuntarily.†

Blood extravasated in the central part of the medulla in the neck, was attended with paralytic affection of the legs, but not of the arms.‡

In a case where the substance of the medulla was lacerated in the neck, there was paralysis in all the parts below the laceration; the lining of the œsophagus was so sensible, that solids could not be swallowed on account of the pain they occasioned.§

Where the medulla in the back was completely divided, there was momentary loss of sight, loss of memory for fifteen minutes, and permanent insensibility in all the lower parts of the body. The skin about the division of the spinal marrow perspired, that below, did not. The wounded spinal marrow appeared to be extremely sensible.‖

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[*] The cranium was not completely ossified, the fontanelle being still of a large size. This case was examined by Mr. Brodie.

[†] A coagulum of blood, the thickness of a crown piece, was found lying upon the external surface of the dura matral covering of the medulla spinalis, extending from the fourth vertebra colli to the second vertebra dorsi. The medulla spinalis itself was uninjured.

[‡] The sixth and seventh vertebræ colli were dislocated; the medulla spinalis externally was uninjured; but, in the centre of its substance, just at that part, there was a coagulum of blood nearly two inches in length.

[§] The seventh vertebra colli was fractured, and the medulla spinalis passing through it was lacerated and compressed.

[‖] The spinal marrow within the canal of the sixth vertebra dorsi was completely destroyed by a musket ball. The person lived four days.

ON THE INFLUENCE OF THE NERVES UPON THE ACTION OF THE ARTERIES.

BY SIR EVERARD HOME, BART. F. R. S.

THAT the pulsations of the arteries correspond in their frequency with the contractions of the left ventricle of the heart, is universally admitted; and those pulsations continuing in the arteries after the limb to which they belong is rendered paralytic, has led to the belief, that all arterial action is independent of nervous influence.

The object of the present paper is to shew that the nerves which accompany the arteries, regulate their actions, and it is through their agency that the blood is distributed in different parts of the body.

The facts which have led me to conclude, that this office is performed by the nerves, I shall lay before the society in the order in which they occurred.

An officer, who had been wounded by a musket ball in the leg immediately below the knee, came under my care. With a view to find the ball, a seton was passed in the course it had taken, and brought through the skin just beyond the part where it was lodged; a caustic was then applied to the skin just below the tuberosity of the tibia, to which the ligament of the patella was attached. It produced great pain all around the joint and through the leg, and what was very remarkable, the matter in the canal surrounding the seton rose and fell with great violence; this circumstance induced me to feel the pulse at the wrist, and to keep my finger upon it during the time this effect upon the pus continued, which was several minutes, corresponding with the pulse at the wrist, which, however, had been in no degree disturbed.

The increased action of the arteries, rendered evident by the effect upon the pus, following so closely upon an irritation, on the neighbouring nerves, made it clear that it arose from that cause, and brought to my recollection an instance, in which the aorta had been seen pulsating with great violence, in consequence of an irritation upon the nerves of the urinary bladder.

To ascertain whether such a connexion between the actions of the nerves and arteries could be demonstrated in a state of health, as well as disease, I instituted the following experiments, which were made in the presence of Mr. Brodie and other gentlemen competent to form a judgment respecting them. The carotid artery of a dog was laid bare, the intercostal nerve and par vagum, which form one bundle, were separated from it by a flattened probe for one tenth of an inch in length, the head and neck were then placed in an easy position, and the pulsations attended to by all present, for two minutes, that the eye might be accustomed to them in their natural state, the nerve passing over the probe was then slightly touched with the kali purum. In a minute and a half the pulsations of the exposed arteries became more distinct; in two minutes the beats were stronger, and in three, more violent; in four minutes the violence was lessened, and in five minutes the action was restored to its natural state.

This experiment was repeated with the same results upon a rabbit, and in that animal the par vagum was separated from the intercostal nerve, which cannot be regularly done in the dog; and it was found when the par vagum alone was irritated, no change took place in the action of the artery. The carotid artery was chosen as the only one in the body of sufficient size, that can be readily exposed, to which the nervous branches supplying it can readily be traced from their trunk.

This experiment was repeated three different times, so as to leave no doubt respecting the result. Having ascertained by these experiments that the increase and diminution of the action of an artery does not depend upon irritability, but nervous influence, I made the following experiments, to determine whether heat or cold had the greatest effect in stimulating the nerves to action.

The wrist of one arm was surrounded by bladders filled with ice, and after remaining in that state five minutes, the pulse was felt at the same time in that and in the opposite wrist, and the beats were found evidently strongest in the cooled wrist. This experiment was then made with water heated to 120° or 130°, beyond which the heat could not be submitted to, and the pulse was found to be softer and weaker than that in the other arm.

When one wrist was cooled and the other heated, the stroke of the pulse in the cooled arm had great force beyond that in the heated one.

These experiments were repeated upon several young men at different ages, and on several women, with an uniform result, and explain the glow produced by the cold bath and the other beneficial effects of cold bathing, in a more satisfactory manner than has hitherto been done.

This influence of the nerves upon the arteries throws considerable light upon some of the most important actions in the animal economy. By its means the same arteries at different times, allow very different proportions of blood to pass through them, and those employed in furnishing blood for the secretions have the supplies regulated, which explains the use of the system of nerves, with which the blood-vessels of the viscera are so abundantly furnished.

The erection of the penis, produced by a particular state of mind, is one of the effects of this influence of the nerves upon the arteries, and the stoppage of the secretions from the same cause is another of an opposite kind.

The ready supply of blood to a limb by the small anastomosing branches when the principal arterial trunk is obliterated, depends upon the same cause, and the coagulation of the blood in the arterial trunk leading to a mortified part, arises from the nerves having lost their influence over it. On this dominion of the nerves over the actions of arteries depends the growth of the body, the regeneration of parts in those animals in which it occurs, as lizards and others, and the formation of tumours of all the different kinds. The circulation of the blood is therefore no longer to be considered as wholly dependent upon the heart and elasticity of the arteries, for, although by these alone it can be kept up, the action of the nerves is necessary to regulate the distribution of the blood to the different parts of the body, according as supplies are wanting to carry on the necessary operations of the animal economy.*

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* From want of room we have been reluctantly compelled to omit some of the notes illustrative of some of Sir E. Home's observations, which make the substance of the first paper.—EDITOR.

REVIEW.

Sketches of Epidemic Diseases in the state of Vermont, from its first settlement to the year 1815 ; *with a consideration of their Causes, Phenomena, and Treatment. To which is added, Remarks on Pulmonary Consumption.* By Joseph A. Gallup, M. D. 1 vol. 8vo. Boston, T. B. Wait & Sons, 1815.

A VOLUME of original matter extending to four hundred pages, and from a single author, is a rare production in our medical world. From this circumstance alone the book before us would claim a large share of attention.

In calling this a volume of original matter, it is not designed to intimate that it contains only new thoughts and new views; but only, that it contains the observations, reflections and opinions of the author; that it is not in any sense a compilation. Dr. Gallup does not pretend to be prepared for his task by extensive erudition, though he is evidently conversant with most of the prevalent opinions in relation to the subjects, which he discusses; but he seems qualified for his undertaking by great strength of intellect; and his observations derive weight from having been founded on extensive experience. His views are bold and comprehensive; his conduct simple, energetick, and decided.

In some preliminary observations, the author exposes the motives and objects of his labours. His work is then divided into ten chapters, which relate to the following subjects:—Chapter I. Topographical sketches of the state of Vermont. II. History of epidemic diseases in the state of Vermont. III. On the causes of fever. IV. On fever in general. V. On the treatment of fever. VI. On spotted fever. VII. On epidemic peripneumony. VIII. On dysentery. IX. On angina epidemica. X. On typhus epidemicus, or continued fever. There are added, "Remarks on Pulmonary Consumption."

The country of Vermont is mostly mountainous; yet, in no part, so elevated as to be destitute of trees and shrubberry. On the east, it has a large portion of meadow washed by Connecticut river. On the west, it has considerable level country in the vicinity of Lake Champlain. The principal epidemics have appeared as much on the hills, as on the low grounds. Intermittent and remittent fevers only are peculiar to these last, near Lake Champlain.

It is since the peace of 1783, that this country has been generally settled. In 1800, the number of inhabitants was 154,449; in 1810 it was 217,915. The diseases, which can be called epidemics, and which have occurred in this state since 1783, have been influenza, *angina epidemica*, pleurisy, peripneumony, dysentery and fever. This last has appeared in some of its principal varieties, both alone and in combination with inflammation.

In 1807, the influenza prevailed in Vermont and throughout the United States. From about this time, indeed previous to it, the disease since called spotted fever, began to appear in New-England. Having already extended its ravages very widely in Connecticut and Massachusetts, it shew itself in Vermont in January 1811. It prevailed also in the following year, mostly in the winter and spring. In the autumn of 1812 and in the subsequent winter, a disease, called by our author *epidemic peripneumony*, made great ravages. This was the most severe disease which ever appeared in Vermont. Its affinity to spotted fever, will be noticed hereafter. Both diseases prevailed in the winter of 1814, and indeed they had before been contemporaneous. In the summer and autumn of 1814 continued fever, generally called typhus, spread extensively in Vermont and in other parts of New-England. At the same time there were cases of angina maligna, dysentery and other diseases. In December 1814, at which time the author closes his account, the spotted fever was said to be appearing in various places. We know that it has, since that, had many victims in New-England.

These are some of the details in the two first chapters; they are selected from among others of great interest, as giving a general view of the later epidemics in Vermont. In the third chapter, the causes of fever are discussed with a good deal of ability; and although all the opinions advanced may not be received by the most learned and most judicious, yet there are many, and those of the greatest practical importance, which may be pronounced perfectly sound. We feel more disposed to comment on some of these, than to controvert such as are of a different description.

In the second section of this chapter the author gives an opinion that diseases, properly called epidemics, are not contagious. For this opinion he advances several reasons, which, if not satisfactory to the most scrupulous, are certainly of great weight; they accord in part with the sentiments advanced in this Journal on a former occasion;* and they serve at least to point out important distinctions between epidemics and those other acute diseases which often spread very extensively, but which are acknowledged by all to be contagious. The reasons are as follow: "The diseases of acknowledged contagion," that is, such as are acute, "have a specific character, are of a certain duration, and affect people but once in the course of their lives." In all these respects epidemics differ from them. Epidemics "cannot be propagated except under certain states of air." The author would have been more correct, since he would not have involved any hypothesis in his statement, had he said that epidemics prevail only at certain times and places from the influence of certain unknown causes, which appear to be general in their operation. Diseases, which are indisputably contagious, are not confined to any periods of time, nor to any regions. It is true, however, that some seasons appear to favour the propagation of these diseases more than others. Further, in many instances, epidemics affect only persons of certain classes, such as natives, or strangers, children, or adults; a distinction not remarked in respect to the contagious diseases. Epidemics much more rarely affect all, or nearly all the members of a family and their visitors than diseases acknowledged as contagious. The author gives his testimony in favour of the opinion, that the attendants on the sick, at the period of an epidemic, are not affected more frequently than those who are not at all conversant with the diseased; except so far as can be fairly accounted for by the exposure of the attendants to exciting causes; such as fatigue, cold, anxiety, and depression of mind. He states also, that persons living in the woods, far removed from the abode of any other human beings, are often affected with an epidemic without having been previously exposed to any one affected with the same during the prevalence of the disease. It may be added, that the period after exposure, at which an acute contagious discase takes place, is almost precisely the same in all cases, while nothing like this can be discovered in the progress of epidemics.

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* See review of Bancroft on yellow fever, in the first volume.

On the question, whether that disease which is designated in this country by the name of typhus, is contagious, as most believe, Dr. Gallup decides in the negative; and after other reasons for his decision, similar to those stated above, he adds one which has always appeared* to us of great force; and that in respect to epidemics generally. This is that, in the case of epidemics, the disease may be arrested by art in any individual case. This may sometimes be done in advanced periods of the disease, but almost always in its earliest period. When was this done in a case of acute disease of an indisputably contagious origin?

The considerations which have now been suggested, do not indeed prove the negative; that epidemics are not in any sense contagious; that persons exposed to the sick, are not at all more liable to the disease than those not so exposed. Let it be acknowledged that instances now and then occur, in which one is tempted to believe that an exposure to the sick has an influence in propagating epidemic fever; and although such instances might be fairly explained on other principles, yet let them have all their due weight upon the prudent in inducing them to avoid unnecessary exposure. Yet, after allowing all the weight to instances of this kind, which they ought to have, we must conclude with Dr. Gallup, that epidemic fever does not correspond in its characteristics and its laws of propagation with diseases generally acknowledged to be contagious; and the same remark may be extended to other epidemics.

For the production of fever, at least of epidemic fever, the author believes that a certain pestilential condition of the air is a primary requisite. This he is disposed, with Mr Webster, to attribute to the influence of comets, an opinion we shall not discuss, or certainly not at this time. This predisposing cause, he believes to act very generally over our globe. He thinks, however, that disease is seldom, if ever, produced in man by this condition of the air without the aid of "local or conjunct causes." These causes he ranks under the names of *miasmata* and *mephitis*; and it is by their agency he explains the limited extent of epidemics, while the pestilential air is spread over the whole earth. Under the name of *miasmata* he includes the supposed causes of disease, both from vegetable and animal matter in a state of fermentation, and also those supposed to be furnished from living bodies confined in jails, ships, &c. Under the name of mephitis, he includes exhalations of mineral origin, issuing from the bowels of the earth. He does not here refer so much to the well known mephitic gases, the carbonic and the hydrogen gases, as to others known only by their morbific effects, and which elude not merely the senses, but the nicest tests of chemistry. In short, there are certain *local* causes, occasioning diseases to be epidemic now in this country and now in that; and our author suspects that these causes issue from the earth "silent and invisible," and not capable of being detected by any chemical test yet known. It is, at least, difficult to refute this suspicion; but it does not lead to any practical consequences. Knowing that the cause is local would have all the effect of knowing that it is produced by a gas which is not cognizable by any other than its morbific effects.

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* See Vol. I. p. 280, of this Journal.

The causes which have been mentioned, that is, the pestilential condition of the atmosphere, and the "local or conjunct causes" act in combination to occasion a predisposition to disease. This predisposition however is specific, the kind of disease being determined by these causes. Yet it is still necessary, at least in almost all cases, that exciting causes should be applied. But these are not specific; they are the same in all epidemics, and have not any agency in determining the kind of disease. These exciting causes are such as occasion considerable derangement in the ordinary functions of the system. Among the principal ones may be enumerated an interruption of the cutaneous functions, indigestion, fatigue of body, anxiety of mind, &c. An important inference to be made from this doctrine is, that during an epidemic, men should be cautious to avoid all these derangements, however well they may ordinarily bear them. These opinions we do not state as originating from Dr. Gallup; it is only designed to add his authority to that of others, whom sound experience has led to adopt them.

In the fourth chapter, we are led into some of the greatest questions in pathology, those respecting fever. If it be impossible to assent to all the author's opinions precisely as stated, it may at least be said that he is always near the truth, and commonly quite within its limits in respect to every thing of practical importance. His description of the phenomena of fever is rapid and animated, and gives abundant evidence of the fidelity of his observations. It accords in several important particulars with the description given by the accurate Fordyce, with whose invaluable work on fever we suspect Dr. Gallup has not been acquainted. It is to be regretted that he has not; since some of his observations and opinions would have gained precision from that truly philosophical source. The opinion that inflammation always accompanies fever, would probably be modified after reading Fordyce's dissertations and Beddoes on the combination of fever and inflammation. The difference between idiopathic fever and the sympathetic affection of the system, produced by great inflammation, would also be more clearly conceived.

Dr. Gallup's remarks on the very intimate and powerful sympathetic relations between the several organic viscera, as manifested in disease, are worthy of high commendation. (See page 158 of his book.) They are among the evidences of his great accuracy and closeness of observation, characteristics of a powerful mind. Other instances of the same kind might be selected from this chapter and pointed out to the attention of readers; but our limits forbid so much detail.

In considering the treatment of fever, Dr. Gallup states, that "three cardinal objects are presented for primary consideration."

"The first is, to consider whether the first impressions upon the system, which have a debilitating effect, are in danger of abstracting the vital principle, and inducing sudden death.

"The second is, to consider whether the vehemence of febrile commotion is in danger of immediately exhausting excitability generally, or of destroying the function or structure of any particular organ essential to life.

"The third is, to consider if the preceding states have passed by, whether the continuance of febrile excitement is in danger of depraving the integral state of the solids and fluids, rendering them unfit for the restoration of healthy action; which event must immediately or ultimately terminate in death."

To counteract the first danger, which occurs only in fevers of the worst character, the subject must be placed in bed and protected by clothing, while the surface of the body is stimulated by friction, warmth and moisture. Meanwhile there should be administered to the stomach active cordials, such as are grateful and pungent to the taste, but which are transient in their effects, not producing permanent heat nor greatly affecting the powers of the system. By such treatment, which is more fully detailed in the work itself, the patient may be brought to the state in which the powers of the system may be exercised. But there is one obstacle to the full developement of the functions in this case, which is noticed by many writers, and which is peculiarly great in spotted fever; or rather in cases of fever where the symptoms of the access are very severe. This is, that in consequence of the suspension of the functions of the secretory and nutritive systems, and of the contraction of the capillaries from which these systems are immediately supplied with blood, this fluid becomes accumulated in the heart and in the large vessels around it. As the powers are brought into more free exercise by the methods pointed out, the heart endeavours to relieve itself of its load. It is at this moment in many instances, that chills and rigors follow the deadly coldness and torpor which belong to the access of the disease. The chills and rigors are very commonly confounded with the coldness and torpor of the attack, or access; but by those, who have observed fever most closely, they are distinguished, as they are by Dr. Gallup. In most cases of continued fever the symptoms both of the access and of the cold stage of febrile paroxysm are very mild, or entirely wanting; or, when they come on, they do so in a gradual manner, and they are so intermixed as that they cannot readily be distinguished. But in the ephemeral fever of Fordyce, in intermittents and in remittents when severe, those symptoms may be clearly distinguished. The same is true in some of the worst cases of continued fever. In all these cases, if the rigors be severe, it serves to show that the system is making great efforts to overcome the symptoms of the access, and that that paroxysm will not be fatal. This has been particularly noticed in intermittents. If the efforts be successful, the blood is now thrown upon the extreme vessels; and by consentaneous efforts in these vessels, efforts which they are enabled to perform by the general developement of the vital powers, and to which they are excited by the blood thus thrown on them, there ensue the phenomena first of the hot stage of fever and subsequently those of the crisis, in a degree more or less perfect. But the obstacles opposed to the free action of the heart are not always so readily overcome. In some cases the depression of the powers, which takes place in the access, continues; or the accumulation of blood is greater than the heart can throw off. At the same time congestions begin to take place in some of the great organs, in the brain, perhaps, or in the lungs; and there arises not only an increased difficulty to the heart from this circumstance, but also danger of ultimate and serious injury to those organs. In this case, which is not very common, but which it is very important to understand, Dr. Gallup recommends a practice calculated to shock many of our good brethren who think only of debility; but one certainly reasonable in itself, which he declares to have been successful, and which, under precisely the circumstances he mentions, ought, we think, to be adopted. It has not, indeed, fallen to our lot to see fever under these circumstances so often, that we can adduce any experience on the subject. The practice proposed by Dr. Gallup is to take away blood, and thus to lessen the impediment to the regular action of the heart and to relieve the loaded organs; at the same time that we are endeavouring by external and internal means to excite both the heart and the small vessels to action, and to promote the warmth of the surface. This is not, indeed, a case in which he bleeds freely. He advises to abstract "perhaps two, four, six, or eight ounces, according as the patient may bear it. If he is a little faint, it is nothing but what is common; a little time will remove it, as the blood-vessels contract. The patient should always be warm, and in a recumbent posture, when bled at this stage, which is supposed to be the first stage, and suffering under oppression. If the pulse is very small and trembling, it is no serious objection to taking blood; nor, in fact, if the faintness should be of considerable duration, will it be an objection to taking more blood, soon after the recovery from this condition. Patients bear a second bleeding, in this condition, better than the first; and the blood flows more readily. Large bleedings would not be proper at this time, but they should be repeated until pain, distress, and coma, are relieved. Proceed with caution; but be not over cautious of a little blood, when the safety of the patient requires it. The congestions must be prevented or removed, and the free circulation promoted in the minute vessels, or all is over. What will do it? Cordials, stimulants, tonics, have all been tried; yet no real permanent benefit arises. The patient may indeed sometimes feel a little more comfortable from a dose of opium, and a gill of wine, for a short time; but in every serious case he is sure to be left worse, unless, peradventure, *the case may not be so severe as was imagined,* and along with this the other curative means may have had a happy effect, and at length, he is found better; and at the same time very willing to impute the benefit to the most agreeable remedy; and not only this, but is willing to attest his sincerity, and prove the skill of the physician, by repeating the dose;—it is done, and again: but shortly neither knows nor cares what is done!"

The reader must look to the work itself, for a full discussion of this practice, and for a statement of the evils resulting from powerful stimulants instead of it. On this point, judicious and learned men differ. In 1810, the great majority were in favour of the powerfully stimulating practice, and those who opposed it, could scarcely obtain a hearing;* but since that time, the number of respectable witnesses against it, has been constantly increasing, and it is grateful to find among them one so intelligent and so discriminating, as the author of the work before us.

Let it suffice, that the practice recommended by this writer, at one of the most critical periods of fever, has been stated with approbation. These pages will not afford room for a full display of his whole method. He does not lay aside his lancet in the subsequent periods; but the conditions under which he uses it, and the considerations which authorize its employment must be learnt from a perusal of his book. In respect to particular opinions and details of treatment there may be room for difference, but the course recommended generally seems to us highly worthy of commendation. The author does not profess to have exhausted the subject.

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* The first considerable opposition to this practice, as we recollect, was made in the report to the Counsellors of the Massachusetts Medical Society, published in 1810. The leading principles contended for in that report are confirmed by Dr. Gallup, yet one would judge that he never saw that paper, as he does not refer to it on any occasion. There is this difference; the report was drawn up by gentlemen who knew the characteristics of the disease only from the statements of others, or certainly with very limited opportunities of personal observation. They viewed the disease in a mirror and reflected from a great distance. They had not opportunity of testing their principles extensively by clinical experiments. The situation of Dr. Gallup was the reverse of all this; and from his intimate acquaintance with the disease, as well as by his descriptive talents, he was enabled to pourtray its features in more just proportions and in more lively colours than was done by the Committee who drew up that report. From the same causes, he is enabled to make better discriminations in respect to the treatment, and to urge his opinions on this point with more confidence than they could do.

The sixth chapter is *on spotted fever*. The subject should make it one of the most interesting in the book; and it is so in respect to its details. But all the great questions relating to it are previously discussed in considering the subject of fever generally. Those who study the natural history of fever, who become familiar with the symptoms which belong to it, with the different stages of its paroxysms and their characteristics; who learn its tendency to produce congestions, hæmorrhages and inflammations; who know that sometimes it *terminates* in either hæmorrhage or inflammation, and that still more frequently it goes on in combination with this last affection; those who have taken these views of fever, and have well regarded them as they have been described by the best writers, will find nothing essentially new in the epidemic, which has attracted so much attention among us. It is to be lamented, that instead of inquiring only whether the disease was sthenic or asthenic, whether it was malignant or pestilential, those who have discussed this subject should not have compared it with other epidemic fevers, noted its differences, availed themselves of the knowledge acquired by experience in respect to the principles which should govern the treatment of fever, and applied these principles to the case before them. In thus viewing the disease they would have found that, while in this as in other epidemic fevers, there were varieties among the cases, the principal characteristics of the disease in its worst form were these; great severity in the symptoms of the access, all the powers of life being depressed in a very great degree, and this in the most sudden manner; coldness arising from a suspension of the functions, but without rigors, as the efforts of the system are not sufficiently powerful to produce these; less than the usual tendency to the hot stage even where rigors take place, but with a more favourable course whenever either rigors or heat do ensue; and lastly, in bad cases at least, congestion often followed by inflammation in some of the great organs, commonly in the brain. These appear to be the circumstances, which distinguish the most exquisite cases of the epidemic called spotted fever, when compared with continued fever of more common and regular character.

The treatment, which is peculiar to the case, has already been suggested in treating of fever generally. We cannot give powers to the system; we can only influence its functions, or promote the exercise of its powers. In the case described, we should first endeavour to promote the actions of the heart and those of the extreme vessels; for on the renewal of their functions depend not only perspiration, but the developement of the powers of the heart and of all the other organs. This purpose must be effected by stimuli applied at once to the skin and to the stomach, by stimuli which act suddenly, but not so permanently as to increase unduly the symptoms of the hot stage. Secondly, and during the use of these remedies, we should relieve the heart, if it be found unable to remove the load opposed to it. This is to be done by blood-letting with the cautions pointed out by Dr. Gallup. And finally, we should guard against congestion and inflammation by evacuations and by counter-stimuli. If, at length, the disease pursue the more common course of continued fever, the phenomena of its successive stages being developed, those remedies should be employed with which experience has made us acquainted. Without taking up the subject precisely in this way, Dr. Gallup has arrived at results very nearly correspondent with those above stated. It is not attempted here to follow him in the minutiæ either of the disease or of the treatment; in which, however, he has evinced both industry and sagacity, and to which we beg the attention of medical practitioners.

In the seventh chapter a disease still more formidable than spotted fever is the subject of consideration. This is called, by the author, *epidemic peripneumony.* It has been much known by the name of *pneumonia typhoides.* Here, as in the other disease to which it is nearly allied, the debility which is a symptom of the disease, has been too often thought to be its cause. In this epidemic, the symptoms of idiopathic fever have been less obvious than in the other. They have been obscured by the symptoms attendant on the local affection of the thorax. Yet, it should be remarked, that in like manner the symptoms of cerebral affection are so strong in some cases of spotted fever, as that they might be confounded with cases of simple apoplexy. In this peripneumony then there was fever combined with an affection of the lungs. This latter affection was probably a congestion only in some cases; but in most instances it was inflammation. The seat of this was sometimes the pleura, sometimes the cellular membrane or parenchyma of the lungs, sometimes the mucous membrane, sometimes the pericardium; often two or more of these membranes were affected at once. The inflammation was excessively violent in degree, and most rapid in running through its several stages. The processes of these stages, therefore, were very imperfectly performed. In the serous membranes the adhesive process commonly failed, and suppuration ensued. Add to this, that great congestion took place in the inflamed parts and in their vicinity, especially when the disease affected either the cellular or mucous membrane.* This is what has been learnt by observing the phenomena during life and after death.

For details of treatment, which are too important to be stated briefly, the reader is again referred to the work. To excite action in the extreme vessels is as important in this disease at its commencement, as in the spotted fever. By so doing inflammation is sometimes removed, but less frequently than congestion; and as inflammation attended this epidemic almost in every case, it required blood-letting in a much larger proportion of cases than the disease called spotted fever. Indeed, it was necessary in every severe case, and it was necessary that it should be liberal. Even this was not enough. The disease was most rapid in its course; a delay of a few hours gave time for great effusions, and it was therefore requisite to repeat the evacuation at very short intervals, until its object was effected; that is, until it was obvious that the inflammation was undergoing resolution. After this was done fever might continue, but for the most part without danger. It was happily the case, in both the diseases which have been considered, that if the first danger was averted, and if the great organs were preserved from injury, the fever was mild in its subsequent periods. To this remark, there were indeed a few exceptions.

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* It is not improbable, that in some cases the air cells were filled with coagulable lymph. We have reasons for this suggestion, which we shall not now stop to detail; it is thrown out only to direot the eyes of observers more accurately to these parts.

The three following chapters, though interesting, must be passed over, and a few observations only will be hazarded on "The Remarks on Pulmonary Consumption." Thus far only the excellencies of the work have been noticed, and even many of them have necessarily been passed by in silence, while its defects have scarcely been glanced at. In this part of his book it must be said that Dr. Gallup seems to us to fall into the error common to all men, and most so perhaps to those who are able to speculate boldly. The fault is in pushing their analogies too far. Benefit often results from this error, but it is nevertheless an error.

Dr. Gallup has looked on the subject of fever with the eye of a master. We think he has something to learn even on this subject; yet it is not inconsistent to say that he has taught much to others. But he endeavours to extend his notions of fever to other diseases. His doctrines would lead him, though he does not seem aware of it, to the opinion maintained by Dr. Rush of the unity of disease. He appears to consider all the great inflammations as the effects of fever, or sometimes as local fever; and to consider chronic inflammation and some other chronic diseases as fever prolonged. It is on these general grounds that he establishes certain principles respecting pulmonary consumption. He considers the affections of the lungs as resulting from an affection of the whole system; and considers it, in all cases, as similar to acute inflammation of the lungs, or pneumonia, except in the rapidity of its processes and in its duration. An ingenious man will always find resemblances among two diseases, when he tries to do so; and by regarding these alone he may persuade himself that such diseases are only modifications of each other. A large portion of physicians in this country and in England, for the last twenty years, have been of the opinion that the actions performed by the living system in disease, differ from those performed by it in health only in degree. According to their system health consists in a certain degree of excitement; if the excitement be either more or less there is disease. Now it seems impossible that men should examine with any caution and fidelity the actions performed by the system in disease and not perceive evidences that these actions differ in kind, as well as in degree. Thus far Dr. Gallup no doubt would agree with us. But then he, having discovered one kind of modification of the actions in disease, is ready to persuade himself that the same modification extends to all diseases. It would require too long an excursion into the domains of pathological science to examine minutely, and to oppose fully, the opinions of Dr. Gallup on this subject. Suffice it to say that he does not seem to us to have made out his case. There are varieties of kind, or specific varieties in inflammation which he does not appear sufficiently to have recognized. And inflammation is very frequently a local disease, even when severe, and affecting great organs, and resulting from constitutional disposition. In truth it is always a local disease; but sometimes it arises in, or is produced by fever, as has been before stated; and, however produced, when considerable in degree it occasions a sympathetic affection of the whole system. This sympathetic affection is not fever, though often confounded with it. These are designed only as hints, and are not offered as a full exposition of principles.

In respect to the treatment of pulmonary consumption, Dr. Gallup offers much that is worthy the consideration of medical practitioners. To do this subject justice the varieties of the disease ought first to be well considered. The circumstance most to be regarded in this author's method of treatment is the frequent use of the lancet. On this point we are disposed to pay great deference to his experience, and are happy that this experience authorizes so much hope in one of the most disheartening evils of humanity. His practice is bold and may be hazardous; but in so deplorable a malady it is lawful to incur the risk of evil. It is not a new opinion with us that blood-letting has been too little practised in this disease in New England. In some trials benefit has resulted, but not in all. Possibly the failure may have arisen, as Dr. Gallup believes, from too much timidity.

In taking our leave of this work it would be unjust not to express anew the pleasure derived from its perusal. A full view of its various merits has not been attempted, and while this was the case we did not feel at liberty and were not at all disposed to cavil at small faults. The style of thought displayed in the work is worthy the highest commendation; and in the expression of these thoughts there is in general not only perspicuity, but great beauty. Yet there are errors both in the language and in the phraseology, which must offend the critic. The work might have been rendered more perfect both in matter and in form, if the author had kept it back for a year or two after it was written, and had occupied that time not only in occasional reviews of it, but also in endeavouring to ascertain what others had written on the subjects it embraces. As he was about to offer a book to the world he will not say it was too much to inquire what the world was already enabled to offer him on the subject of his book. This probably did not occur to him; besides, he saw the immediate necessity of enlightening those about him. This consideration authorized some haste, and we should not therefore have been induced to make these remarks, but that they bear upon a statement in the preliminary observations, which shall be the last subject of comment. It is there stated, that "in the first period of practice, after traversing the paths of disappointment, the writer imbibed a disrelish for books, and laid them mostly aside for several years." This mode of conduct was not singular. It has been adopted by many, and it would hardly seem worthy of comment were it not that the author rather justifies it now, in his maturer years; and that his brilliant success may induce too many to follow the example. Now this course of conduct seems to us very pernicious. In the majority of cases it it is fruitful of evil. Men who cease to study for a few years seldom begin again; and they most commonly study nature very negligently, when they no longer study books. To prevent disappointment too much should not be expected from books. Let students learn anatomy with the knife in their hands, and under professed masters of the science; let them study physiology, positive physiology; that is, all that is really known respecting the living healthy body in all its parts; then let them learn the general principles of hygiene, pathology, and therapeutics; when thus prepared let them read the descriptions of diseases and the modes of treatment. They will thus acquire most important knowledge, but do not let them believe that they are now prepared for practice. Dissections, lectures and books are not enough. They will necessarily read much which they will not understand at first. They must now bestow all diligence to obtain an acquaintance with the phenomena of disease at the bed-side, and there patiently endeavour to trace out the lines which have been described, while they modestly correct the delineations when not exact.

That something like this is requisite for the education of a physician will hardly be denied. But will it be contended that from this time his study of books may cease? The truth is that the longest term allotted for a medical education is insufficient for him to peruse all the writings, encumbered as they are with extraneous matter, of men, who, like Dr. Gallup, have enriched our science by giving the results of their experience. The observations of such men cannot be obtained in the abstract from a medical dictionary, a compendium, or vade-mecum. They must be studied in the original writings of the great observers; and to understand those writings it is necessary to acquire a knowledge of the doctrines of the age in which they were written. Such works too are perused and re-perused to most advantage after a man has become familiar with the visage of disease and its habitudes. Add to all this the necessity of keeping alive in the mind and of frequently reviewing the great fundamental principles of physiology, pathology, &c.; of enlarging, retrenching and correcting them; add also the necessity of following the course of improvement in various parts of the world in the days in which we live; and after all this, instead of laying aside books, it only remains for us to lament that the time employed and fatigue produced in the practice of our profession, leaves so few hours in which the physician can avail himself of the aid, which books will afford him. Let us hope that these opinions will more generally prevail; and although we may often wade through volumes of useless speculations, or of ill-digested observations, yet the reward will be sufficient if once in a year we can be enriched by instructions such as are furnished us in this work by Dr. Gallup.

Des Erreurs Populaires, relatives à la Médecine. Par M. Richerand, Professeur de la Faculté de Médecine de Paris, &c.

Odi profanum vulgus, et arceo.—*Horat. Carm. lib.* 3, *od.* 1.

PROFESSOR RICHERAND is sufficiently well known to the medical public, as the author of a popular work on physiology, and a system of surgery, which, in imitation of professor Pinel, he has distinguished by the name of "*Nosographie Chirurgicale.*"

The little work before us is perhaps more creditable to the understanding of M. Richerand, than any of his productions; though one disposed to find faults might consider his references to ancient authors, and his quotations from the Latin poets as more numerous than was necessary. These faults will not prevent the medical profession nor the public from feeling grateful to this author for his assistance in delivering them from the errors of the day. Some of our best and latest writers on the philosophy of the mind have assured us, that a principal object with every man should be to unlearn a considerable part of his education, in order to free himself from errors.

"Scarcely," says M. Richerand, "is the infant disengaged from the bonds which united it with its mother, and introduced to the light, than Error, this queen of the world, seizes it and ranks it in the number of her subjects." If, in the course of a laborious parturition, the head has changed its ordinary form, the anxiety of those around is immediately excited and every exertion is employed to bring the part into a more agreeable shape. They are not aware, says M. Richerand, that nothing less than a pressure, long continued, is capable of producing a permanent change of form; and that their forcible efforts are calculated to disorganize the structure of that delicate viscus, the brain. Six months at least must the head of the infant be bound up to produce the forms so much desired by the *Macrocephali*, the *long heads* of ancient Thrace, or the *flat headed* Indians of the present day.

The question whether the infant should be washed in warm or cold water is one of no small importance, and M. Richerand therefore traces the practice back to the ancient Spartans, wholly omitting, however, to take notice of the mother of Achilles. The Spartans, it seems, plunged their new born children into the icy waters of the Eurotas; for which they had two good reasons—the first, was to harden those who were vigorous and well constituted, and the second, to get rid of the weak and sickly. Although this practice was proper among the Spartans, a nation who considered themselves as born only to fight, he thinks it would not be judicious at the present day, because we find among the puny and sickly, such as the Lacedemonians would have made away with in infancy, some of our greatest statesmen and finest wits. M. Richerand therefore prefers warm water to cold, for young infants, and to this may be added a little soap, the detergent qualities of which are extremely salutary to new born children.

The second part of this work treats of errors relating to health and its preservation. This leads to the question, what health is? A question not so easily answered as might be supposed, for we are informed that this desirable state has never been well defined.

After disapproving of the ridiculous application of the term *non-naturals*, to those things which affect health, M. Richerand adopts this same division, and treats separately on each part of it, viz. air, food and drink, clothing, exercise and repose, the excretions, sleep and watching, and the passions.

Under the first head he remarks, that the practice of making great fires to purify the air, is founded on error. Because, as he thinks, the contagion of disease is propagated by solid substances, such as cloths, cotton, wool, &c. Although his reasoning on the subject is not satisfactory, the truth of this opinion cannot be doubted; since whether the diseases, called contagious, are propagated by contact; or produced by a peculiar atmosphere, great fires cannot have much influence in changing either of these circumstances. On this subject, he might have adverted to that practice, by which the olfactory and pulmonary organs of the physician are frequently saluted with the burning of rags, paper, and sugar in the chambers of the sick. In this attempt to destroy one kind of bad effluvia by another, the noxious atmosphere is neither changed nor decomposed, but is loaded with an addition of filthy and unhealthy particles. Vinegar fumigations do not merit the same reproach.

The precept of the school of Salernum

> "Post prandium sta, post cœnam ambula,"

is not agreeable to the opinion of M. Richerand, because there is, after eating, a natural disposition to a state of tranquility and repose, in order that, the other parts of the system being at rest, nature may employ all her energies to support the exertions of the stomach. He appears to have strong prejudices in favour of sleeping; and gives a piece of advice which may be very agreeable to some of us, that those, who are not strong, should never oppose the inclination to sleep, however it may return frequently. Our brethren of the faculty, who are compelled to active labours a great part of the time, and to devote the remainder to acquiring and retaining a smattering of science, might perform a part of this latter business while they sleep; for, as he informs us, when the senses are relieved from external objects, they do not trouble the understanding, which then has an opportunity of exerting itself most completely on the objects of memory, and passes from one step of reasoning to another, till it arrives at the most profound and occult truths. Hence, he concludes, that it is very possible for us to prophecy in our dreams.

This second part is concluded with the following remarks on popular treatises on medical subjects, "These advices to the people on their health are books made by mediocrity for ignorance. Never did a master of the art, never did a truly illustrious physician debase himself by works of this kind. Tissot's Advice to the People, and Buchan's Domestic Medicine, have cost more people their lives than the most murderous war. The reading of such books cannot be too severely prohibited the people; for they are filled with errors or with ideas that are incomplete, and of course liable to lead to error, in a science, whose practical application is so delicate. Ignorant of the foundations on which precepts are supported, they never take into account the innumerable modifications relating to age, sex, temperament, season of the year, climate and other differences, so numerous, that we may say in medicine, the cases of exception are almost as frequent as those to which the rule will apply. They inspire the most presumptuous and the most culpable confidence in persons, whose zeal is as blind as it is charitable."

The third part treats of errors relating to diseases. Here again the author commences with a display of his erudition, which carries him to the time of Apollo, Mercury, Trismegistus, or Hermes, Isis and Osiris, and Cham the son of Japhet, and from these ancient times he drops easily into the inquiry, how it has happened that for so many ages, people have been ignorant of the manner of treating the most trifling cuts in the fingers? The praises of balsams, salves, and healing herbs, have been sung ever since Homer's time. It cannot therefore surprise us, that as soon as a man is wounded, people run with balsams, which they pour into the wound; but they irritate and inflame it, and prevent its healing at once, by these extraneous substances. Nature, left to herself, would do much better for the patient; and nature assisted by art will perform wonders. When the two sides of a cut in the flesh are near each other, nature pours from the extremities of the divided vessels the coagulating lymph, an animal glue, which in the course of a few hours cements the parts together. In a simple, clean cut, there is then nothing to be done, but to place the wounded parts fairly together, and secure them, in order that the lymph thrown out, may not be disturbed and the sides of the wound separated anew. This is performed by means of bandages and plasters; not because there is any peculiar virtue in these applications, but because they are most convenient to secure the wounded parts in their contact. We solemnly warn all those, who may read these pages, never hereafter to stuff a fresh wound with lint balsams, salves, or cerates. It is proper to avoid being too curious to examine the state of a recently dressed wound; for if it is opened on the second day, the tender union will be dissevered, the first intention of nature will be defeated, and she, having been disappointed by our folly in her first attempt, will be compelled to try a second way of repairing the injury. Matter will be formed to wash away the blood and lymph, and granulations, or small grains of flesh will appear, which grow together and fill up the wound, almost in spite of our most industrious applications to prevent.

The popular prejudices against mercurial medicines are adverted to by M. Richerand; and he shows the absurdity of supposing that this substance can remain in the system for some time after it has ceased to be given; for as every part of the system is continually changing, where can the mercury find a resting place. Does it not on the contrary, by increasing the secretions, make for itself a passage out of the body? Is it not discharged in the saliva, the alvine excretions in the fluid of perspiration, &c. What folly then to suppose, that it can exist for years in the living body. And is it not equally absurd to follow a course of mercury with purgative medicines, which can have no effect but to increase the patient's weakness? Richerand, although he thinks the use of mercury too much dreaded in France, is ready to consider it as a cause of consumption; especially when used in the form of the muriate of m. or corrosive sublimate. This medicine, he is confident, is the essential article in all the secret remedies of syphilis, the famous *vegetable* syrups, and antisyphilitic robs ; an opinion of his which would be equally correct with regard to our country. In spite of all the secret ways in which mercury is administered in France, it is probable that, if we except its use in the venereal disease, there is more mercury employed in one large town of the United States, than in the whole of that great nation. To what extent this excessive use can be justified by the number of diseases cured it is almost impossible to devise means to ascertain ; and of course we have no right to expect to be relieved from our doubts on the subject. But while we continue to see it cure a variety of diseases, in which the effect of other remedies is imperceptible, but that of mercury is evident, we must continue to employ it ; unless indeed it shall appear, that while it cures the present disease it lays the foundation for a future. It is absolutely necessary, however, to use it with more caution than is generally practised. Richerand well remarks, that a true salivation is not necessary even in the venereal disease. It is sufficient, at least in the greater number of cases, to obtain the slightest possible symptom of an affection of the system. Even if the appearances of disease can be removed without such symptoms, we presume no one should consider it necessary to push the patient to a salivation. In febrile complaints this medicine should be employed with great caution, and with an attentive eye to its effect on the intestinal canal, on the general system, and also to the peculiarity of the season. For it is certain, that in some seasons this medicine affects the constitution, in a less favorable manner than in others. In the diseases of children, mercury is often employed, and in truth with safety and advantage, to a very great extent. In young subjects, the mischievous effects of the medicine are less frequent than in those more advanced. It must be noticed however, that in some of them, it produces great debility in the time of its administration, and in others its very liberal use is occasionally followed by a paralysis of one or more of the extremities. Such instances are extremely rare, and do not form an objection to the use of the medicine, with due caution and dicrimination.

Bone setters, rupture doctors, cancer curers, and all kinds of quacks, are scarcely less common in France than in other countries. The venders of quack medicines are exposed to an inconvenience, which one would suppose likely to be fatal to their trade. Every inventor of a new medicine is compelled to submit it to the examination of a medical commission, and make known its composition, before he can obtain permission to sell. M. Richerand has frequently been a member of these commissions, and has had an opportunity of observing that most of the quack medicines are copied from ancient and forgotten books. In this country, it is not necessary to go so far back; for the simplest and most common medicinal substances, neatly put up in a sealed bottle or box, will be swallowed as greedily as the most occult remedies of Paracelsus, provided it has a promising title. The common people, for want of knowledge, see these things with the eye of their imagination, which diffuses a mysterious atmosphere around every thing unknown. Nor are these alone liable to be deceived. When we see that some of the most gross dupes of quackery have been men of great understanding, we learn the necessity of not trusting ourselves out of our proper circle of knowledge.

The following remarks relating to an important and painful part of the duty of a physician, are given in the words of M. Richerand. "One of the most burdensome duties imposed on the physician is that of announcing to the relations of the patient that his disease will prove fatal. Nothing is more dangerous and more delicate, than to intimate the same thing to the patient himself. Whatever may be the importance of the civil or religious arrangements, which remain for him to make, it is always cruel to the eye of feeling and even of reason, to poison the small relique of existence with the terrors of death. Nature, like a tender mother, conceals from man and from other animals the knowledge of their end. The consumptive patient, devoured by a slow fever, and the dropsical, almost suffocated by a collection of water, still dream of their approaching cure, and sleep sweetly, while thus cradled by the consoling illusions of hope. The patient, who, from his uneasy curiosity, seems to you the most resigned, would be mortally affected, if he saw his physician agree with him in opinion. Announce to him that his fears are not without foundation, you will see him turn pale and his anxiety will be strongly painted in the midst of a forced resignation. The disturbance he feels may soon increase to delirium. We then hear the storm of passions, not yet extinguished, or the murmur of regrets which the most magnificent promises of religion and the most touching exhortations cannot always calm." "A physician raised streams of blood, mingled with pus; he was wasted by sweats and reduced to a frightful degree of emaciation. If, said he to me, I attended a patient, who presented such a collection of symptoms, I should not hesitate to declare him to be affected with the last degree of phthisis; but I am perfectly satisfied of the contrary. It would have been barbarous to undeceive him. All physicians do not experience such happy illusions, and to know the certainty of one's fate is a pain connected with the study and practice of a profession, in other respects so painful."

This work is concluded with remarks on the actual state of the practice of the medical art, and the qualifications of its professors. The separation of the department of surgery from that of medicine, which has taken place in France and great part of Europe, has arisen from the experience of many centuries; for it has been found impossible, that all physicians should possess opportunities enough for operations, to obtain that peculiar state of mind, which is requisite for an operating surgeon. It has been found vain for a young man, who may have qualified himself with a good knowledge of anatomy and made a collection of brilliant instruments, which he occasionally contemplates with great satisfaction, to expect to acquire at once he talent for operating. The preliminary knowledge must be followed by experience, long and uninterrupted. This alone will enable surgeon in the midst of his operation to possess that stat of mind, which allows him to observe every accident and meet every danger. The duty of an operator is like that o a military officer. He must previously arrange his regular plan of oper ion, and then contemplate all the extraordinary circumstances, which may occcur and require the aid of extraordinary means. When the operation begins, when the blood gushes over him, and the cries of the patient are ready to distract him, he collects his faculties the more closely, as he finds them more assailed and disturbed. His mind is elevated, by the occasion, above its ordinary powers. He sees every thing, provides for every thing, and proceeds, with a steadiness which nothing can interrupt, to the accomplishment of his object. In the practice of medicine, experience is not less necessary to the formation of that quality, which the French call *the tact.* M. Richerand defines the tact to be "that precious quality granted or refused by nature, which is in medicine, what taste is in literature. It depends on a peculiar sensibility, perfected by education." This tact does in reality depend on *experience* principally, though the natural or artificial talent for observation is necessary to its acquisition. By this the physician distinguishes the disease of his patient amidst a crowd of confused symptoms and contradictory accounts, and he distinguishes it often with a quickness, which surprises himself. By this, he sees at once the proper and principal remedy. In respect to this tact, M. Richerand thinks the French to be incontestibly superior to all other nations. Though less learned than the Germans, their quickness, their coup d'œil renders them much more able. The Germans, according to him, though a cold and selfish people, are the most liable in the world, to be wrong in medicine, because they are constantly running after some new doctrine. "At present," says he, "they are divided between the doctrine of Brown and that of the new chemists. The greater part of the systematic physicians, such as Van Helmont, Stahl, and Hoffman, wrote in Germany." "Since the Monads of Leibnitz, Germany has seen the rise and fall of an infinite number of metaphysical sects; and of late Wolf, Swedenborg, and Kant, have successively governed their philosophers." "The Italians, our elders, and the English, our true rivals, appear to me to be far superior to them in all parts of the sciences, arts and literature." This little display of national spleen and vanity is partly well founded, and altogether amazing.

"This is the place," says he, "to blame exclusive practices." M. Richerand means no doubt to allude to the fondness of the Germans for exclusive systems. "Nothing is more common than to find physicians, who employ the same remedy in all diseases, so that we can tell before, what methods they will recommend to those, who ask their advice." One practitioner cures every thing by bleeding; another by emetics; a third loves purgatives, and others have each a favorite medicine, which they are sure to employ on every occasion, during a certain time; that is, till they become tired of it and have found something new. Of the same nature is another error, which he might have placed by the side of the last. That of seeing the same disease every where. This is scarcely less frequent than the last, and may serve as the foundation and excuse for it. By the use of an exclusive remedy, which is to be held up and praised loudly on all occasions, some persons will now and then succeed in attracting the public notice. One may give soda, a second potass, and a third ammonia, and all do equally well, with a proper talent. Of the truth of this, we may see a curious instance in our vicinity, in the great reputation, which a person has acquired among the vulgar, by uniformly prescribing the simplest and most harmless substance, the stagnant water of our cisterns. All this is unworthy the attention of one, who wishes to enjoy the consciousness of exercising his faculties to the greatest advantage of his fellow men. "There is," says M. Richerand, "a sure ground for reputation to physicians, who are really worthy of that name. Nothing is capable of shaking it, when it rests on that immoveable foundation. I mean to say, *the judgment of their peers*, the esteem and consideration of their brethren. Although envy and detraction too often alter these judgments, they are, in general, as just as they are enlightened."

Why is it that the profession of medicine, more than others, has been the object of the satire of the witty and the ill-nature of the stupid? M. Richerand thinks, "that these sarcasms have generally issued from incurable patients, who imputed to the medical art, the wrongs which nature had inflicted on them. Montaigne was an invalid, Molière a hypochondriac and habitual raiser of blood, Rousseau continually tormented with pains in the bladder." It is to be recollected, however, that they satirized a set of physicians, who, according to M. Richerand's confession, deserved nothing better." Their formal dress, their affected gravity, their gross ignorance, and their absurd and horrible farrago of prescriptions did not entitle them to respect. Some persons have supposed that people hate physicians, because they are associated with the pain and humiliation, which they experience in sickness. The truth is, that when physicians are well educated and treat their patients with care and tenderness, and inform them, as far as circumstances admit, of the nature of their disease, and the probable operation of the remedies, then they will receive their confidence and love. Such physicians may merit and enjoy the compliment which the repentant Rousseau paid the profession, when speaking to M. Bernardin St. Pierre, in the latter part of his life :—"If I was to make a new edition of my works, I would soften all that I have said about physicians. There is no condition which requires so much study as theirs. In all countries, they are the most truly learned men." We are not to assume such praises to ourselves, but we should endeavour to deserve them.

Medico-Chirurgical Transactions, vol. v. London, 1814.

We have the pleasure to present our readers with an analysis of the fifth volume of this highly valuable work. A passing compliment to this latest volume of the Transactions of the Medico-Chirurgical Society, would neither add to its reputation, nor to our character as reviewers. We offer our readers the following abstract as the choicest gift we could make them :

Art. 1. *Case of Periodical Jactitation or Chorea.* By Robert Watt, m. d. President of the Faculty of Physicians and Surgeons at Glasgow, &c. &c. &c.

The first case is that of Mary Wardrop, aged ten years, rather delicate, but active, and a very good scholar for her age. What is remarkable in this case is the variety in the movements of the body, which occurred during the disease. She was, at first, seized with most excruciating headach, and required to be kept constantly in an erect posture, as the only one which procured her the least alleviation of pain. This continued with occasional variety in the symptoms to the first of February, one month from its commencement, when she was seized with a propensity to turn round on her feet like a top. This motion was incessant from morning till night, and the more violent, the more comfort she enjoyed. At the end of February this motion ceased. Headach again came on, most violent in its symptoms, which deprived her of speech and the use of the muscles of the neck. At the end of March, these symptoms subsided, when she was seized with a new sort of motion, and again she lost her speech. The motion was a rotatory one, commencing from the top of the bed and continued to the bottom, the assistants moving her back, when she had arrived at the bottom. At first, the fits continued two hours each day, but gradually increased to six or seven. Her friends took her to the garden, and she rolled from one end of a walk to the other, and continued this motion in a shallow part of a river, when she was almost at the point of being drowned. She made but little or no use of her arms in turning; indeed, they were often rigidly extended by her sides, as in tetanus. She has been known to revolve in this way fifty or sixty times in a minute. She continued this motion for six weeks. About the end of April, she was seized in a different manner; the motion consisted in drawing her head and heels nearly together, "bent herself up like a bow, then allowing them to separate, her buttocks fell with considerable force on the bed." This she did at first for six hours, but, gradually increasing, she at last continued it for fourteen hours a day, at the rate of ten or twelve times in a minute. This continued for about five weeks, and was succeeded by an entire new one. This was a propensity to stand on the head, or rather, nape of the neck and shoulders. This was done, from twelve to fifteen times in a minute, for fifteen hours a day.

Early in the month of June, Dr. Watt first saw her. Her father gave the above history of the disease. This case now becomes still more interesting to us, as it is the record of a respectable physician. Our limits, however, will only allow very general histories, and we must pass over much very interesting matter. The disease appears to have gone on unimpeded by the medical means, used with a view to palliation or cure. The father had it in his power to assist the natural termination of the paroxysm which took place in the evening by forcibly grasping her, placing his hand on both sides of the chest, lifting her from bed, placing her on his knee, and holding her fast. This, however, never succeeded at any other part of the day, or prevented the morning accession of the paroxysm. A spontaneous diarrhœa after some time occurred. She became more tractable; purgatives were exhibited and perfect recovery took place. Dr. Watt observes, "I cannot help thinking that the present case goes a considerable length in proving the expediency of Dr. Hamilton's purgative plan in chorea and similar affections."

Art. 2. *Case of Abscess in the Brain.* By Alexander Denmark, M. D.

This case was fatal. The patient had received a blow on the head with a piece of wood which merely stunned him. He came under medical treatment for inflammation of the ear, attended with purulent discharges, without fever, on the eighth of August; on the thirteenth, the symptoms of diseased brain occurred, which were fatal the 31st, and dissection discovered cerebral abscess.

Art 3. *A Practical Essay on Hemeralopia or Night Blindness, commonly called Nyctalopia, as it affects Seamen and others, in the East and West-Indies, China, the Mediterranean. &c. &c. in which a successful Method of Cure is Detailed.* By Mr. R. W. Bampfield, Surgeon to the Royal Navy, &c. &c.

According to Mr. Bampfield "the distinguishing symptoms of hermeralopia, are a partial or total deprivation of the sight from the time of the setting of the sun to its rise on the following morning, while the sight is distinctly preserved in the day time. The disease always affects both eyes at the same time."

Its progress is in general gradual, and when neglected it may advance not only to loss of vision at night, but to the degree of total blindness. It appeared that idiosyncracy had no effect in inducing it. In general, the constitutional health does not suffer in idiopathic cases. The eyes do not always exhibit the appearance of disease. At times, however, they are rendered very susceptible, and suffer from various degrees of light, various degrees of distress. The remote causes of this disease are not well ascertained. Bontius imputes it to eating rice; and this is a popular opinion in the East-Indies. Dr. Pye thinks the disease an intermittent one. Exposure to strong light while asleep, has been considered among its remote causes. "Hemeralopia," says the author, "is certainly a periodical disease, inasmuch as it returns every day after sunset; but there is no feature in its history or character, that has induced me to be of opinion, that its period of return is influenced by the same causes as intermittent fever." He ventures to conjecture it may be the consequence of exposure to the vivid light of tropical climates, by which the retina is rendered unsusceptible of the weaker light of night. Scorbutic hemeralopia is referred to the changes which the humours of the eye undergo in common with all the fluids of the body, as its cause; and the opinion is supported by the fact, that when the state of the solids and fluids is made better by treatment, the hemeralopia is also diminished. What is of far more importance than all we have said on this subject, is the declaration of Mr. Bampfield, that "of more than one hundred cases of idiopathic, and two hundred of symptomatic hemeralopia that have occurred in my practice, all have perfectly recovered; and hence it is inferred, that under proper treatment the prognosis may be always favourable, and the patient may be at once assured of the restoration of his sight."

The treatment is exceedingly simple. It consists in a succession of blisters to the temples of the size of half a crown piece, applied tolerably close to the external canthus of the eye; and this, according to the author, has succeeded in every case of idiopathic hemeralopia he has seen. Our limits will not allow us to detail the phenomena produced by blisters.

For symptomatic hemeralopia, our remedies are to be found in those used for the idiopathic disease under which the system labours, and of which the affection of the eyes is but the effect. For that which attends scurvy, the remedies of scurvy are to be given, and should the blindness not yield to them, treat it as idiopathic, viz. apply blisters. Electricity has been used with success. At times this disease is cured by the spontaneous efforts of nature; sometimes suddenly, at others gradually. We regret that we have not room to accompany this very intelligent author through his critical remarks on Bontius, Scarpa, &c. &c. Though it is with less reluctance we leave the pursuit, as he has furnished such simple and effectual means for the treatment of the disease.

ART. 4. *A Case of Cartilaginous Substances successfully extracted from the Cavity of the Knee-Joint.* By John Clark, M. D. Surgeon to the Forces.

IN this paper are detailed three particular operations for extracting cartilaginous bodies from the knee-joint. Three bodies of this kind were removed, and perfect motion restored to the joint.

Art. 5. *Account of the Extraction of a Loose Substance, consisting partly of bone and partly of cartilage, from the Elbow Joint.* By James Millman Coley, Member of the Royal College of Surgeons in London, &c. &c.

The operation was performed with the usual cautions, and a flat substance extracted nearly of the shape of the vomica nut, (strychnos nux vomica, Lin.) It consisted of bone of a brittle quality, covered with cartilage. In a few days the wound was healed, and the movements of the joint made with perfect ease.

Art. 6. *Account of a Chemical Examination of the Urine and Serum of the Blood of a Person who had been taking large quantities of Soda.* By John Bostock, M. D. of Liverpool.

Our limits only permit us to give the following very imperfect extract from this highly valuable paper. The urine was found to be composed as follows :

	Entire Urine.	*Solid Contents.*
Water	750	
Urea united to a fixed alkali. Muriate of ammonia.	142 5	57
Phosphate of soda. Muriate of soda.	93 75	37 5
Posphate of lime Albumen.	13 75	5 5
	1000 00	100 0

Art. 7. *Case of Laceration of the Internal Coat of the Stomach and Duodenum, by Vomiting.* By Thomas Chevalier, Esq F. R. S.

The subject of this case, aged fourteen, was seized with vomiting on Sunday evening after having eaten heartily, and drank rather freely of gin and water. The vomiting continued till Tuesday evening, when he died. Towards evening he vomited, in successive efforts, near two pints of blood. On dissection it was found that the internal coat of the stomach and duodenum had been torn in various places. The lacerations were much larger in the duodenum than in the stomach ; and near the pylorus they extended nearly round the circumference of the gut, and rendered this part so weak that it was quite torn off in a very slight effort to loosen this portion of the intestine for the purpose of passing a ligature round it. Two extensive lacerations were also found near the middle of this portion of the intestine.

Art. 8. *On Contractions after Burns or Extensive Ulcerations.* By Henry Earle, Esq. Surgeon to the Foundling Hospital.

This is a very interesting paper. We agree with the author, "that these contractions are a source of blame to surgeons." We feel under obligations to him for a mode of practice "to remedy some of the unfortunate effects of the contractions to which burns are so very liable." Frequently, however, such contractions do not depend on any inattention on the part of the surgeon, but are the result of a natural process which follows cicatrization, and which has often baffled all the efforts of art to prevent. This process consists in an absorption of the granulations on which the new skin has been formed; by which the cicatrix is made to occupy a much smaller extent than the originally ulcerated surface. Perhaps it would be speaking more correctly to say, that the granulations which are at first florid and extremely vascular, after having deposited the new skin, receive a smaller proportion of blood, become paler and diminished in bulk, and consequently occupy much less surface for the new skin.

From the ill success which the author had observed to follow the usual operation for the contractions and deformity of burns, he was led to adopt a new one; and this consists in a removal by dissection of the cicatrix, and then to endeavour to approximate those portions of the integuments which were most easily brought together. The operation was done on the arm of a lad six years old. From the fore part of the upper arm to within two inches of the wrist, a firm tense cicatrix of an almost horny consistence, extended, which kept the elbow immoveably bent to a right angle. The whole of the cicatrix, except a triangular flap, which Mr. Earle conceived might be useful in accelerating the healing of so extensive a wound was removed. The arm was next extended. This was effected with considerable difficulty, from the forcible contraction of the flexor muscles. The edges of the wound were now approximated as much as possible, and the whole limb bound down on a splint. On account of the length of the operation and the pain, and a great disposition to spasm, a large dose of opium was given the patient. The next day the arm was brought perfectly to a right line. The cicatrix which was left on the limb sloughed. This piece had been detached from every part, except its upper broad basis. The operation was performed on the twelfth of November, and the whole arm was healed by the twentieth of December. Perfect motion in the limb was restored. "Should a similar case" (observes Mr. Earle) "occur to me again, I should prefer the removal of the whole cicatrix at once, to leaving it with the probability of sloughing."

Art. 9. *The History of a Child Retained in the Mother fifty-two years after the usual period of Utero-Gestation.* By Rutland Brown Cheston, M. D. F. R. S.

"We may assert," says Dr. Cheston, "without fear of contradiction, that the whole range of physiology and pathology does not afford a more striking instance of the resource of nature, than the effects consequent on an interruption of the natural passage of the ovarium into the uterus.

"This history exhibits a fœtus, after arriving in the uterus from the seat of its original formation, remaining nine months, and acquiring the full size in that organ, on the point of being expelled from its cavity by the process of parturition, when it escapes in an almost incomprehensible manner from the uterus, and becomes surrounded by a bony covering, in which it is found at the end of fifty-two years almost as perfect in its form, as we should have expected to find it, if it had been born at the natural period of utero-gestation."

Such, in a few words, is an account of this mysterious case in the words of the author himself, and is a faithful abstract of the details contained in eighteen pages of this interesting volume. Two other instances similar in many of their circumstances to the above, are mentioned by the author. From these he hoped to have gained some light on some curious circumstances which remain to be explained; such as the reduction of the tumour; the remarkable diminution in the weight of the child, and the time at which the bony cyst was formed. One of the women is still living; in the other, no intreaties could overcome the obstinate prejudices of the husband against an examination of the parts after death. In this last, the labour had commenced, subsided, and the child retained in the mother five years. After this interval she became again pregnant, labour regularly occurred. "Upon examination I found a round solid substance, occupying the upper part of the pelvis, and extending from the base of the sacrum, to within less than three inches of the upper rim of the pubes." This was not the head of the child. The child was delivered piece meal. Inflammation took place, and the woman, after having discharged various bones of the former fœtus, after having lingered in the most cruel sufferings for about ten months, died.

Art. 10. *An Account of some Diseases of the Toes and Fingers, with observations on their Treatment.* By James Wardrop, Esq. F. R. S. Edinburgh.

The first of these diseases mentioned by Mr. Wardrop, is that which is generally denominated "The Growth of the Nail in the Flesh." This affection is chiefly confined to the great toe. We have only room to transcribe the treatment. The disease is readily recognized. It consists in an inflammation, swelling, and suppuration in the soft parts contiguous to the nail. The treatment of this disease consists simply in the application of caustic to the diseased parts about the nail. This soon relieves the pain, diminishes the swelling, in short, cures the disease. To prevent its recurrence, care should be taken not to cut off the corner of the nails, particularly that of the great toe; for when this is done, the shoe presses the adjacent soft parts against the sharp edge of the nail, and thus produces pain and inflammation. The next affection treated of is onychia maligna, or ulceration at the root of the nail. The circumstances attending this disease are, first, swelling of a deep and red colour of the soft parts at the root of the nail. An oozing of thin ichor next occurs, from between the root of the nail and soft part. Ulceration takes place at the circular edge of the soft parts at the root of the nail. The ulcer is unhealthy, edges thin and acute, its surface covered with a dull yellow, or brown coloured lymph, and attended with an ichorous and very fœtid discharge. The growth of the nail is interrupted, it loses its colour, and, at some places, appears to have but little connection with the soft parts. In this state, the author has seen the disease continue for several years, so that the toe or finger became a deformed bulbous mass.

In this affection, Mr. Wardrop found from the internal exhibition of mercury a most beneficial effect. He had tried the various local means without any benefit. But, when the constitutional effects of mercury were produced, the condition of the ulcers was improved, simple ointment was applied and the disease soon cured. After the sore began to heal, a weak solution of the muriate of mercury was used. The internal use of that remedy was continued till the ulcers were perfectly healed, and, as is generally advisable under such circumstances, it was taken in smaller quantities for some time after the patients were apparently cured.

The next affection treated of is corns. We can only mention the treatment. This consists simply in paring away as much of the corn as is safe; to keep the toe immersed for some time in warm water; and after drying all the skin contiguous to the disease, to rub carefully all the surface of the corn with lunar caustic, or wet it with the solution of muriate of mercury, by means of a camel's hair pencil. Two or three applications will be sufficient to cure the disease; and should it re-appear, the same means must be resorted to again.

For chilblains, next mentioned, the treatment is also very simple. It consists in the application of a mixture of one part of tincture of cantharides to six parts of the soap-liniment. Use this embrocation once or twice a day, and afterwards keep the part warm. Where ulceration has occurred, dress the ulcer with simple ointment, and the neighbouring inflamed parts with the cantharides. There is no application so useful to the ulcer itself as the common ointment, composed of the red oxide of mercury.

(To be Continued.)

INTELLIGENCE.

Officers of the Massachusetts Medical Society for the following year.

Joshua Fisher, M. D. *President.*
Thomas Welsh, M. D. *Vice President.*
David Townsend, M. D.
Aaron Dexter, M. D.
Josiah Bartlett, M. D. } *Censors.*
William Spooner, M. D.
James Jackson, M. D.
John C. Warren, M. D. *Corresponding Secretary.*
John Dixwell, M. D. *Recording Secretary.*
John G. Coffin, M. D. *Treasurer.*
John Gorham, M. D. *Librarian.*

On Friday preceding Commencement, the public examination for the degree of Doctor of Medicine was held in Cambridge, and the following candidates, having read and supported their respective dissertations, received their degrees on Wednesday:

John P. Batchelder, of Charlestown, New Hampshire, "*On Aneurism of the Heart.*"

Samuel Burnham, of Ipswich, county of Essex, "*On the connection between the Brain and Mind.*"

Henry W. Farley, of Ipswich, county of Essex, "*On Suspended Animation.*"

Josiah F. Flagg, of Dover, New Hampshire, "*On Suspended Animation.*"

Moses Gage, of Rowley, county of Essex, "*On Phrenitis, or Inflammation of the Brain.*"

Pliny Hayes, of Boston, "*On the treatment of Fever.*"

George W. Heard, A. M. of Ipswich, county of Essex, "*On the Sympathy between different parts of the Body.*"

John Homans, A. M. of Worcester, "*On Ulcers.*"

Calvin Peck, of Castine, District of Maine, "*On Dropsy.*"

Edward H. Robbins, jun. A. M. of Milton, county of Norfolk, "*On the Symptoms, Causes, and Treatment of Acute Rheumatism.*"

Ezekiel Thaxter, A. M. of Abington, county of Plymouth, "*On the state of Medicine in China.*"

Solomon D. Townsend, A. M. of Boston, "*On the Disease produced by Mercury.*"

John W. Webster, A. M. of Boston, "*On the Diagnosis between Hydrocephalus, Worms, and Foulness of the Stomach and Bowels.*"

Joshua B. Whitridge, A. B. of Tiverton, Rhode Island, "*On the Epidemic which prevailed in the Northern Division of the Army in the autumn of* 1812 *and winter of* 1813."

The degree of honorary Doctor of Medicine was conferred on the following gentlemen.

Abraham Haskell, of Leominster,
Benjamin L. Oliver, of Salem,
Oliver Prescott, of Newburyport,
John D. Treadwell, of Salem.

Medical Lectures.

THE winter course of lectures in the Medical Institution of Harvard University will commence in Boston, on the first Wednesday in November.

Anatomy and Surgery, by Dr. Warren.
Chemistry, by Dr. Dexter and Dr. Gorham.
Materia Medica, by Dr. Bigelow.
Theory and Practice of Midwifery, by Dr. Channing.
Theory and Practice of Medicine, by Dr. Jackson.

JOHN C. WARREN, M. D. is chosen Professor of Anatomy and Surgery in the University at Cambridge.

Jacob Bigelow, M. D. is appointed lecturer on Materia Medica, and Walter Channing, M. D. lecturer on Midwifery, in the Medical Institution of Harvard University.

THE new College of Medicine, which the liberality of the state legislature has enabled the University to erect in Boston, is now raised and the roof nearly covered. This building is about ninety feet long and forty-five feet wide. It is to contain three ample theatres for Lectures, rooms for a Museum, Library, Chemical Laboratory, and every thing which is necessary and convenient in an edifice of the kind.

Bandages in Rheumatism.

DR. BALFOUR, of Edinburgh, has published in the Edinburgh Medical and Surgical Journal, for April, 1815, a long paper on the good effects of tight bandages in chronic rheumatism. The first part of this paper is made up of physiological and pathological observations, which we have not room to insert. We have selected some cases from the latter part, which will give our readers a sufficient view of his practice in this disease.

" On the 15th December, William Graham, aged thirty-two, a private in the Cumberland militia, complained of violent *lumbago*, which rendered him unable to mount guard, and he was unwilling to be sent to the hospital. On examining the part affected, found he wore a broad *bandage* round his body. This he had been accustomed to do, but said he experienced from it no benefit to his back. I asked his pocket-handkerchief, which I formed into a compress, and laid it on the part affected, applying the *bandage* over it as tight as he could bear it. On inquiring how he felt, he replied, " Perfectly at ease. I feel a want supplied, and I can turn myself any way I please with perfect freedom." He went to bed quite well, but in the morning found himself much worse. I saw him again on the 16th, about three o'clock afternoon, when I asked him if he had laid aside the *compress* that his pains had returned ? He said he still wore the *compress*, and experienced from it so much benefit, that without it he could not walk at all. On the 15th I omitted to examine his pulse, which I now found at 100. I therefore immediately took twenty ounces of blood from his arm. This took off the phlogistic diathesis of the system, and the compress, which, as the pain had extended, I ordered to be enlarged, completed the cure."

" On the 21st of October last, I was called to a girl of fourteen years of age, whom I found sitting in a warm room, before a large fire. Her skin was hot, face flushed, and pulse at ninety ; all which I was disposed to attribute to the warm regimen she had been so carefully observing. I desired her to remove to a distance from the fire. She told me she could not move, on account of rheumatic pains, reaching from her ankles to the middle of her thighs. This account I soon found to be correct, for when, in consequence of my pressing her, she made an attempt to rise, she was forced to cry out most bitterly, so that I saw her limbs were totally immoveable. I told her I would make her walk through the room, without pain, before I left her. So much difficulty was experienced, however, in removing her from the place where she sat, that I began to suspect I had put my credit to too great hazard, and therefore endeavoured to pass off, as a joke, the assurance I had given of instant relief. Having accomplished the removal of the patient to a proper situation, I applied a roller of flannel, with a degree of tightness which she could easily bear, to both limbs, beginning at the middle of the thighs, and continuing it downwards over the feet. I now requested she would make an attempt to walk. To this she consented, on condition of being indulged with another person's arm. I allowed her to touch gently her aunt's shoulder. The girl rose up and walked through the room, stiffly to be sure, but without complaint. I ordered her to be put to bed, and as she seemed to labour somewhat in her breathing, was desirous of taking a little blood from her arm. This was peremptorily resisted by her aunt, the lady of the house, on the score of the patient's youth. It was in vain I represented, that, though bandaging the limbs was a powerful auxiliary, it never could supersede the diligent use of other remedies generally employed in the cure of rheumatism; above all, that it could have no influence whatever on febrile symptoms. Next day the patient's general health was much the same; the pains as bad as ever. I suspected the cause, and soon found the bandages were hanging loose. I immediately replaced them with the same good effect as before. With the exception of some laxatives, this girl got no other remedy, and the pains were, notwithstanding, put to flight in a few days. A sister of this girl, aged eighteen, soon after my patient got better, was seized in like manner, all over the inferior extremities, with rheumatic pains. Without calling any medical aid, she had recourse to the *bandages*, which had the same happy effects with her as with her sister."

"On the 9th of January, I was called to Mr. B. a gentleman traveller, lodged in the Black-bull inn, who informed me he had been arrested on his journey, for four weeks before, by acute rheumatism, and that he had been attended by ———. His father, who had come from Whitehaven to see and carry home his son, having heard, in the town, of the successful application of bandages in the cure of rheumatism, mentioned, on his return to the inn, the circumstance to the patient, who took no rest till his father brought me to him. I stated the impropriety of my interfering with the patients of another practitioner in his absence. But the young gentleman told me, "he had not seen his doctors for some time; that he conceived they did not intend to visit him any more, he having been out; and, at any rate, he was not to be deprived, on account of any trifling piece of etiquette, of the benefit of the *bandages*, if benefit was to be obtained from them."

This gentleman I found convalescent, but complaining grievously of pain in his left hand, which was also much swelled; of pain and stiffness of the left elbow; of pain and weakness about the left shoulder; of pain and rigidity of the muscles of the back of the neck; of weakness and pain upon motion, all along the spine; and of pain and stiffness of the left knee, the outside of which he could scarcely suffer to be touched.

I began with desiring the patient to shew me how he could walk. He moved and threw his weight upon the left leg, slowly and timorously, as if afraid to trust himself upon it. But no sooner was a bandage applied, than he planted his foot firmly, and walked with great freedom, declaring that, in respect of his knee, he felt quite a different man. I next proceeded to examine the state of his left arm, and found the range of motion it possessed very circumscribed indeed. He could not even put his hand to his mouth. Upon application, however, of a *bandage* from the points of the fingers to the elbow-joint inclusive, he could do it freely. On account of the patient being very much fatigued, I did nothing at this call to the shoulder, farther than grasping it with one of my hands, in order to shew him what could be done. Upon this, he moved his arm with great facility in every direction.

January 10*th*. Passed a good night, having slept from eleven to five without interruption. Pain of the left hand and knee greatly abated; in other respects as before. Removed the *bandages* and ordered the diligent use of the flesh-brush, for a considerable time, to the parts affected, which was extremely grateful to the patient. Applied the *bandages* as before, and also to the left shoulder, making a turn under the right, by which two breadths of the *roller* were made to press upon the back of the neck, a circumstance that also gave much pleasure and satisfaction.

January 11*th.* Pain of the left knee, wrist, and elbow almost gone. Muscles of the neck also much relieved. Swelling of the left hand greatly abated. Motion of the left arm entire, and much more vigorous. Omit the *bandages.*

January 12*th.* Has slight pain and stiffness to-day, in both ankles. In other respects improving. Swelling of the left hand entirely gone. Applied *bandages* to both ankles.

January 13*th.* Was out yesterday; but says the pain and stiffness of his ankles were such, that he could not have walked at all without the *bandages.* Is resolved to have *a set of them* to carry along with him. This, I think, sufficiently indicates the opinion he entertains of their utility. It is, indeed, in such cases as this, that the pre-eminence of *bandages* in the cure of rheumatism is most conspicuous. Let us suppose that *bandages* had not been applied to this gentleman's ankles, he must of course have remained at home; let us suppose that I had confided to rubefacients and flannel, for the removal of his pains, when would the desired effect have been produced? in a day? a week? a month? No man can answer these questions. One thing only is certain, that the longer a patient in such circumstances is confined to the house, the longer is his general health of being restored. But no sooner were the *bandages* applied to this gentleman's ankles, than he was enabled to walk abroad. The consequence was, he returned with an appetite greatly improved, and with a universal glow and moisture of the skin, which are most successfully promoted by action in the open air, and of more real benefit to a *rheumatic* patient, than all the sudorifics in the world."

At the conclusion of the paper Dr. Balfour makes the following summary on the effects of bandages in rheumatism.

"At certain times, in some cases, but especially at the beginning, it is necessary to make the bandages tighter than they can be well borne for any length of time. In such cases, frequent removal of the bandages, and friction in the intervals, are indispensable.

The pains and rigidity of parts do not return immediately on the removal of the bandages; the effects remain for a considerable time after the cause is removed.

Parts that have for a considerable time been treated with bandages come at last to be covered, when the bandages are at any time removed, with a copious, warm, and fluid sweat, which gives a kindliness and pliancy to the skin, which it did not possess before.

Parts that have for a considerable time been treated with bandages acquire plumpness and strength, while parts of the same person, that do not admit of being bandaged, remain emaciated and tremulous as before."

Opium in Uterine Hæmorrhage.

Dr. Sparks informs us, he has derived great benefit from the free use of opium in uterine hæmorrhage, in cases in which the uterus was spasmodically affected; which is attended with grinding pains, sometimes is termed spurious, and at others colic. In those cases, the os uteri contracts instead of dilating; examination gives pain and brings on hæmorrhage, and, until the spasmodic state of the uterus is removed (and nothing but large doses of opium will effect the removal) the labour will be suspended. Repeated small doses of opium will not have the same good effect as a large one.

A case was given, in which this practice was beneficial.

New Medical and Physical Journal.

Puerperal Fever.

In the London Medical Repository for May, 1815, we find detailed five cases of puerperal fever, communicated by Mr. William Gaitskell, surgeon. Dr. Haighton, lecturer in midwifery and physiology at Guy's Hospital, saw one of them in consultation with Mr. Gaitskell. The cases were all very severe, and under the treatment adopted, all recovered. The plan was simple, but bold and decisive. It consisted in bleeding (which was repeated six times in one case, from the 20th to the 22d of February, both days included) till the frequency of the pulse was diminished and a sensible alteration for the better was made in the general state of the patient; and in purging, until the alvine discharges were made better. The author of this paper believes in the highly contagious nature of puerperal fever, and in this opinion he is supported by Dr. Haighton and many others.

Predisposition to Aneurism.

The third paper in the same number contains the history of a case, somewhat remarkable for a predisposition of the patient to aneurismal enlargement of the arteries; one existed in each thigh, and one in the aorta. For that on the right thigh, the common operation was successfully performed.

Oxalic Acid.

It appears by the same Journal that this acid, taken to the extent of an ounce by mistake, instead of Epsom salts, operated like a poison. From various experiments made by the author of the communication, W. Roberts, Esq. surgeon, the following conclusions were drawn:—

1. "That oxalic acid taken into the stomach, acts as a virulent poison; although the exact quantity that will produce this effect be unknown.

2. "That oxalate of lime does not produce any deleterious effect when swallowed.

3. "That a mixture of chalk and water, by producing oxalate of lime in the stomach, when oxalic acid has been previously swallowed, may be regarded as the antidote of that acid when exhibited very soon after that poison has been swallowed."

Gleet.

In the Edinburgh Medical and Surgical Journal for July, 1815, is a case of gleet related by Edward B. Fletcher, surgeon. This gleet was the sequel of gonorrhæa and had continued upwards of two years, notwithstanding in that time he made use of a variety of remedies. The patient being ordered (he was in the army) to the sea coast, he accidentally tried an injection of *sea-water*, by which alone he was cured in ten days.

Cow Pock in Persia.

Extract of a letter from William Bruce, Esq. resident at Bushire, to William Erskine, Esq. Bombay, dated

Bushire, *March* 26*th*, 1313.

MY DEAR SIR,

When I was in Bombay, I mentioned to you that the cow-pock was well known in Persia by the Eliāāts, or wandering tribes. Since my return here, I have made very particular inquiries on this subject among several different tribes who visit this place during winter, to sell the produce of their flocks, such as carpets, rugs, butter, cheese, &c. Their flocks, during this time, are spread over the low country to graze. Every *Eliāāt* I have spoken to on this subject, of at least six or seven different tribes, has uniformly told me that the people who are employed to milk the cattle, caught the disease, which, after once having had, they were perfectly safe from the small-pox, that this disease was prevalent among the cows, and shewed itself particularly on the teats; but that it was more prevalent and more frequently caught from the sheep. Now this is a circumstance, that has never, I believe, before been known, and of the truth of it I have not the smallest doubt, as the person of whom I inquired could have no interest in telling me a falsehood, and it is not likely that every one whom I spoke to should agree in deceiving, for I have asked at least some forty or fifty persons. To be more sure on the subject, I made very particular inquiries of a most respectable farmer, who lives about fourteen miles from this place, named Mallila (whom Babington knows very well) and who is under some obligations to me. This man confirmed every thing that the *Eliāāts* had told me; and further said, that the disease was common all over the country, and that his own sheep often had it. There may be one reason why the *Eliāāts* say that they caught the infection oftener from the sheep than the cow, which is, that most of the butter, ghee, cheese, &c. is made from sheep's milk, and that the black cattle yield very little, being more used for draught than any thing else.

Edinburgh Medical and Surgical Journal.

Mr. Wadd has lately opened the head of an epileptic subject and found the left hemisphere of the brain entirely destroyed by suppuration. The patient, in the latter part of his life, was blind in the right eye. He retained his intellects to the last, and could sometimes express his wishes.

London Medical and Physical Journal.

Mr. Carpue has succeeded a second time in his Taliacotian operation, conducted on the Asiatic plan, greatly improved. The subjects were both military, and their improved physiognomy has been sufficient to attract the notice of the commander in chief and even of the Prince Regent, who has appointed Mr. Carpue his surgeon extraordinary. *Ibid.*

Mr. Cuvier has examined at Haerlem a petrefied skeleton, extracted more than one hundred years ago, from the quarries of Æningen, near the Lake of Constance, which Scheuchzer, a naturalist of Zurich, had taken for that of a man, and which he had engraved as "The man who was a witness for the deluge." M. Cuvier has ascertained that this skeleton belonged to an unknown and gigantic species of salamander, as he had already announced, on a simple view of the engraving, in his great work on fossil animals. From not meeting with the fossil remains of human beings, Mr. Cuvier thinks that man is the newest inhabitant of the globe.—*New Medical and Physical Journal.*

Effects of Severe Cold.

M. Desgenettes, the celebrated French physician, who accompanied the unfortunate army that penetrated into Russia, thus describes some of the phenomena, which occurred among the troops who were exposed to the intense cold, which was fatal to so many thousands, during the retreat from Moscow. "I have heard men," says this acute observer, "who were marching with every appearance of muscular energy, and with the most decided and soldier-like pace, suddenly complain that a thick veil was covering their eyes; those organs at first, for an instant, haggard, became immoveable; all the muscular apparatus of the neck, and more particularly the sterno-cleido-mastoidean muscles became rigid and gradually riveted the head on the right or left shoulder; this rigidity extended to the trunk; the lower extremities tottered, and the unhappy victim fell upon the snow, exhibiting, to complete the frightful picture, all the symptoms of catalepsy or epilepsy."—*Ibid.*

M. de Saissy, surgeon, of Lyons, has obtained success in several cases of deafness by making injections into the hollow of the tympanum, through the Eustachean tube. He has sent to the Mathematical and Physical Class of the National Institute, the description of his method, and the history of the cures which he has effected.—*Ibid.*

Medical Report.

The months of June and July were very dry and hot. On the first of August, the temperature in the morning was very high, but in the afternoon there took place a great change in the weather. In Boston there were very heavy showers, with thunder and lightning, and it was noticed that the clouds were unusually dense and black. In Salem there was an uncommon fall of hail, which was of such size and descended with so much force, as to break many thousand panes of glass in that town. On the same day a hail-storm was experienced at Halifax. The change which took place on that day was very extensive and permanent. The summer seemed to terminate at once. In August and September there were some hot days, but the general character of the weather in these months was wet, cold and stormy. A severe storm happened on the ninth of August, and on the last of August and first of September another, which extended over the northern part of this continent and a great part of the Atlantic ocean. On the 23d of September we were visited by a hurricane, which terminated a northeasterly storm, and which was severely felt along the coast and about eighty miles in the interior of the New England states. In this vicinity it was the most violent which has occurred within the memory of persons now living here. Details of its effects are to be found in all the journals of this period.

From the early part of June, cholera morbus and colic became frequent and continued so through the summer. Towards the end of July, before the termination of the hot and dry weather, dysentery began to appear. This disease increased in frequency and severity during August and the first part of September. It began to decline before the great storm of the 23d, and has been much less frequent since that time, having given place to inflammatory affections of the throat and lungs. Some cases of severe cholera infantum appeared in July and August, but they were not numerous and that disease has scarcely been seen in September. Cases of fever have been rare; and those which occurred in September were disposed to terminate in dysentery.

The cholera morbus and colic affected adults mostly, and yielded very readily to medicine. The dysentery also affected many adults, but the most numerous subjects of it were young children. In these it seemed to take the place of cholera infantum, a disease with which it has much in common. The dysentery is a rare epidemic in this place, although it is sporadic here every autumn. The number of deaths produced by it has been very small. In some of the fatal cases among children, it was attended with convulsions from a very early stage of the disorder. A considerable proportion of the worst cases were in children, who had been spending some weeks or months in the country, and who returned to the town about the end of August, or in September, being seized almost immediately on their return.

In one fatal case, that of a child who had been affected with violent convulsions, an examination of the body was made after death. The mucous membrane of the large intestines was found in a state of inflammation from the valve to the extremity of the rectum. The inflammation was most violent in the lower half. In all this half, and in some parts of the other portion, the mucous membrane was lined with a coating of coagulable lymph. This appearance on the mucous membrane is uncommon. It is the same which so often takes place in croup. The effusion of this substance on the mucous membrane denotes an inflammation both rapid and violent. In a few cases the dysenteric symptoms suddenly subsided, and symptoms of inflammation of the lungs appeared; these again disappeared and gave place to the original disease. These changes have happened more than once in the same patient.

September 30th, 1815.

RECENT ENGLISH PUBLICATIONS.

A Practical Treatise and Observations on the Nature, Variety, and Treatment of the Venereal Disease, &c. &c. By T. Kiernan.

Practical Observations on the Treatment of Gleets and Strictures of the Urethra. By John Andree. 8vo.

The Morbid Anatomy of the Liver, part II. on the varieties of the Tubera Diffusa. By J. R. Farre, M. D. 4to.

The History of the Small-pox. By James Moore. 8vo.

Memoirs of Military Surgery. By D. J. Larrey, M. D. Abridged and translated from the French, by John Waller, surgeon of the Royal Navy. Part I. 8vo.

A Practical Inquiry into Disordered Respiration, distinguishing the Species of Convulsive Asthma, their Causes and Indications of Cure. By Robert Bree, M. D. F. R. S. &c. Fifth edition, corrected and improved, with additional observations. 8vo.

Reflections on Fever, intended to point out the Principles upon which a systematic and useful Method of Treatment might be established. By Robert Calvert, M. D. physician to the Forces, &c. &c. 8vo.

Additional Reports on the Effects of a peculiar Regimen in Cases of Cancer, Scrofula, Consumption, Asthma, and other Chronic Diseases. By William Lamb, M. D. 8vo.

Researches on Pulmonary Phthisis. From the French of G. L. Bayle, by William Barrow, M. D. 8vo.

A View of the Relations of the Nervous System in Health and Disease; containing selections from a Dissertation, to which was adjudged the Jacksonian prize for the year 1813, with additional Illustrations and Remarks. By Daniel Pring, member of the Royal College of Surgeons, London, and surgeon at Bath. 8vo.

Pathological Researches. By J. R. Farre, M. D. Royal 8vo.

A Dissertation on Gun-shot Wounds. By Charles Bell, surgeon. Illustrated by seventeen Engravings. Royal 8vo.

The Morbid Anatomy of the Brain, in Mania and Hydrophobia; with the Pathology of these two Diseases, as collected from the Papers of the late Andrew Marshall, M. D. To which is prefixed a Sketch of his Life. By S. Saury. 8vo.

A Practical Treatise on Porrigo, on Scald Head, and Impetigo, the Humid or Running Tetter, with coloured Engravings, illustrative of the Diseases. By the late Robert Willan, M. D. F. R S. and F. A. S. Edited by Ashby Smith, member of the Royal College of Surgeons. 4to.

Surgical Works of John Abernethy, F. R. S. A new edition. 2 Vols. 8vo.

Engravings of the Thoracic and Abdominal Viscera and the Canals connected with them; representing the natural appearance of those important parts immediately after Death, and without being affected by previous disease. Drawn under the direction of, and with discriptive letter-press by, Alexander Monro, Jun. F. R. S. Royal 4to.

A Statement of the early Symptoms which lead to the Disease, termed Water in the Brain, with observations on the necessity of a watchful attention to them, and on the fatal consequences of their neglect, in a letter to Dr. Wall, of Oxford. By G. D. Yeats, M. D. of Trinity College, Oxford, and of the Royal College of Physicians, London.

TO CORRESPONDENTS.

We have received from Dr. Hale, some remarks on the observations made by Mr. Brodie on his inaugural dissertation. We regret that they did not arrive in time to be inserted in this number. They shall appear in our next.

After Dr. Whitridge's paper was printed, we received from that gentleman a note containing some emendations which the author requested us to make in the original manuscript. The following are the most important.

Saturday, January 14. The anodyne not having the desired effect at *one* o'clock, instead of *ten* as it now stands.

In the second paragraph, page 332, third sentence, the term *ruptured* should be substituted for *divided* artery.

In the same paragraph after the words "their edges smooth," the reader is requested to add, "sides not agglutinated; the calibre of that portion of the artery, from the point where the ligature was made, to where the internal iliac is given off, was filled with a plug of firm coagulated blood."

Dr. Whitridge also wishes to add the following queries to those which are already printed at the conclusion of his paper.

"Upon what principle are aneurisms cured simply by making a ligature upon the artery above them?"

"Why do not aneurismal tumours, afterwards, continue to be supplied with blood, by the anastomosing vessels?"

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